# THE

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## Art. I. *On some Phenomena relating to the Formation of Dew on Metallic Surfaces.* By George Harvey, M G. S., *&c. &c.*

[Communicated by the Author.]

It is a curious fact, mentioned by Dr. Wells, in his valuable *Essay on Dew*, that if a metallic substance be closely attached to a body of some thickness, which attracts dew powerfully, the tendency of the metal to promote the formation of moisture on its surface, instead of being increased from the circumstance, is diminished, provided the metal covers the whole of the upper surface of the body to which it is attached*. This principle he illustrated by the following experiment: Two pieces of very light wood, each four inches long, a third of an inch wide, and one tenth of an inch thick, were joined in the form of a cross; and to one of its sides the non-metallic surface of a square piece of gilt paper was attached, by means of mucilage. On exposing the metallic surface on a dewy night, by suspending it in a horizontal position, about six inches above the ground, he found after a few hours, that the parts of the metallic paper, not in contact with the wood, had minute drops of dew on their surfaces, while those in contact with the cross, were perfectly dry.

In repeating this experiment, I have employed gold and silver metallic paper, attached to frames of various forms; and by prosecuting the subject under different circumstances of the atmosphere, I have met with some interesting and beautiful phenomena, which seem to merit a particular description.

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* Wells on Dew, page 22, second edition.

The metallic squares were sometimes suspended a few inches above the ground, and at other times placed on surfaces of glass, or on the recently-mowed herbage. The particular situation of each will however be noticed, as the different experiments are described.

In endeavouring to trace phenomena relating to the deposition of dew on the surfaces of polished metals, some perseverance is necessary; as it is but seldom that the circumstances of temperature and moisture are such, as to permit its ready formation. It would appear, that not only the depression of temperature, and the presence of moisture in the lowest atmospheric stratum, must be considerable; but that the superficial dimensions of the metal have also an influence on the formation of moisture on it. The difference between glass and polished metals in this particular is singularly remarkable. A small vitreous surface, when presented to a clear and tranquil sky, has its surface as readily covered with moisture as one of larger dimensions; but in the case of metals of the same kind, of polished tin for example, a large metallic plate is sometimes more readily dewed than a small one; whereas, under other circumstances, one of a small area is covered with a copious deposition of moisture, whilst a large one will preserve, during the whole night, a bright and unsullied surface*. I have thought it proper to introduce these remarks, in order to apprize the young inquirer of the disappointment to which he will be frequently liable, when prosecuting this interesting subject with relation to polished metals.

Whenever the squares of silver paper were exposed for the purpose of receiving dew, it was remarked, that the first formation of moisture took place at the corners of the triangular portions of the metallic paper, not in contact with the wood; the particles being exceedingly minute, and requiring the aid of a magnifying glass to discover them. As the radiation of the metallic surfaces was promoted by the influence of the clear nocturnal sky, those particles of moisture gradually increased both in number and size; while other minute drops began at the same time to be deposited on the edges of the square; so that in the course of three hours, the metallic surface had assumed the appearance represented in fig. 1, Plate I. the shaded parts denoting the particles of dew, and the dotted lines the position of the cross to which the metallic paper was attached. After midnight, the farther deposition of moisture appears to have been suspended; as at half an hour before sunrise, the appearance of the metal was nearly the same as when the last observation was made.

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* "A large metallic plate, lying on grass, resists the formation of dew more powerfully than a very small one similarly situated. If a large and a very small plate be suspended horizontally, at the same height in the air, the small plate will resist the formation of dew more powerfully than the large."—WELLS on *Dew, page 22, second edition.*

It was most interesting to observe, during the progressive deposition of the moisture, that the particles were disposed in triangular forms, similar to the right-angled triangles, into which the metallic paper was divided, by its contact with the cross; and this was the case even when the triangles, from their minuteness, might be esteemed of an almost elementary kind. And at the last observation, when the greatest quantity of dew for the night had been deposited, the triangular figures were perfectly well defined, their hypothenuses being bounded by the edges of the metallic surface, and their several bases and perpendiculars, respectively parallel to the arms of the cross. So also the gradual accumulation of moisture in the small segments, whose chords coincided with the edges of the metal, was marked by the same uniform and progressive character; the particles, during their increase in number and magnitude, preserving a beautiful curvilineal contour to the figures which they formed. The parts of the paper in contact with the cross, had no dewy particles on them; their junction with the wood appearing effectually to prevent the formation of moisture; thus confirming the observation of Dr. WELLS.

On another night, favourable to the copious formation of dew, the moisture was not confined to the small triangular and curvilineal spaces alluded to in the preceding experiment, but was diffused over the whole surface, excepting the parts in contact with the wood. The metallic surface presented therefore a dry portion with well defined borders, in the form of the algebraic sign *plus*; and four triangles of dew, formed of particles beautifully distinct, but undergoing a minute diminution in size, from the edges of the paper to the vertices of the triangles, as represented in fig. 2. To contemplate these triangular formations of dew to advantage, it was necessary to place the eye in a situation to receive the impression of the reflected light. In such a position the forms of the triangles were viewed to great advantage; the innumerable atoms of dew presenting a pleasing contrast to the unsullied figure of the cross.

On the same night a similar surface of gold metallic paper, similarly circumstanced, presented an appearance as in fig. 3, in some degree analagous to that represented for the silver paper in fig. 1. The moisture seemed however destitute of that uniformity which characterized the particles formed in the last-mentioned diagram, although some slight approach to it might be traced, in the formation of the irregular patches of dew, in the angular portions of the figure.

On the following night, portions of silvered paper attached to triangular and square frames, as denoted by figures 4 and 5, were presented to the nocturnal sky. In the former, the beautifully minute particles of dew were confined to the equilateral triangular surface, not in contact with the frame; the particles however seeming to preserve an uniformity of magnitude over the whole surface. To the middle of the non-metallic surface of fig. 5, a circular piece of wood was attached, of the same thickness as the frame; and during the abundant deposition of dew which took place in the course of the night, the moisture was strictly confined to the parts of the metallic paper, not in contact with the wood; the small circular portion, although surrounded as it were with an atmosphere of moisture, presenting as effectual a barrier to its formation as the external frame.

During the prosecution of these experiments, I have had frequent opportunities of remarking that silver metallic paper permits dew to be deposited earlier, and in greater abundance on its surface, than gold. Early in the month of April, at nine P. M., a large pane of glass was placed on the green herbage, and on it the squares of gold and silver paper attached to their respective crosses. The clear and transparent sky, joined to the perfectly tranquil state of the atmosphere, indicated the possibility of a copious deposition of dew. At six the next morning the grass exhibited the appearance of a thick hoar frost, and the moisture which had been formed on the upper and under sides of the glass during the night, presented coats of transparent ice. On referring to the squares of metallic paper, that of gold was found removed from the glass on which it had been placed the preceding evening, to the distance of six feet; its change of situation having been probably produced by the force of some breeze during the night. The metallic side was in contact with the grass, and on taking it up it presented four beautiful triangles, completely covered with innumerable particles of frozen dew. Those parts of the metal which had their inferior surfaces in contact with the wood, exhibited the perfect and well defined form of the cross, represented in fig. 6. The appearance of the crystalline triangles, when contrasted with the golden surface ef the cross, was extremely beautiful; and it was remarked, that as the gradually increased warmth of the morning dissolved the crystals of dew, the moisture was still confined to the same triangular surfaces: thus preserving completely the form of the cross. Minute crystalline atoms were also perceptible on the non-metallic side of the paper, and which, likewise dissolving, had a sensible effect on the rigidity of the paper. On examining the silver square which had preserved its situation on the glass, its metallic surface was found without the appearance of moisture, under any form, on its surface.

A hasty consideration of this phenomenon might lead us to infer, that dew is more readily deposited on gold than on silver, contrary to what has been before remarked, as the result of extended observations; but a farther investigation of the anomaly in question may lead to a satisfactory explanation of its cause.

The appearance of the heavens at the time the metallic surfaces were placed in the meadow for observation, indicated, as before remarked, the probability of a copious deposition of dew, during the night, and that a considerable quantity was deposited, the hoar-frost in the morning clearly proved. The temperature, and the hygrometric state of the air, were also, from other collateral circumstances, to be regarded as highly favourable to the formation of dew on metallic bodies; and that a breeze must have existed for some time during the night, sufficiently powerful, at least, to remove the metallic square, together with its attached cross, to the distance of several feet, is likewise apparent. These circumstances will account for the anomaly in question.

In the first place, dew was most probably deposited during the former part of the night, in a sufficiently copious degree, to cover the four triangles on each of the metallic surfaces. This deposition may be presumed to have taken place before the temperature of the lowest stratum of air, in contact with those surfaces, was depressed to that of the freezing point. The breeze removed the golden square, and left its metallic surface in contact with the short herbage, the temperature of which had been previously reduced to 32°. This temperature necessarily caused the particles of dew already deposited on the triangular surfaces to crystallize; and left the cross with its lustre undiminished. The same wind dissipated the moisture that had been deposited on the silver surface; for it has been remarked by Dr. Wells*, that "the dew which has formed upon a metal will often disappear, while other substances in their neighbourhood remain wet." The breeze indeed may have continued the remainder of the night, and prevented any new formation of dew on the silvery surface; but, at the same time, permitted moisture to be deposited on the non-metallic surface of the golden square; because white paper has been placed by the last-mentioned philosopher among the substances that are even more productive of cold than wool*. Or it is possible that the breeze may have subsided, and the circumstances of temperature become such as to have allowed the deposition of dew on the paper, but not of its re-formation on the silver.

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* Page 21, *Essay on Dew*, by Dr. Wells, second edition.

Dr. Wells has also remarked†, that when dew forms upon metals, it "commonly sullies only the lustre of their surface; and that even when it is sufficiently abundant to gather into drops, they are almost always small and distinct." This observation, however, requires some limitation; since, on nights that have been more than usually cold, and when the quantity of moisture in the air has been abundant, I have observed the dewy particles deposited on metals to attain a considerable magnitude; and examples have even occurred of polished tin surfaces being completely covered with thin sheets of water, the result of the junction of the innumerable minute particles deposited on them.

On one night, equal squares (their linear edges being one inch and half) of lead, zinc, brass, copper, and tin, were laid on a large plate of glass, and presented to the influence of a clear sky. At sunrise the next morning, the particles of dew on the different surfaces were found of variable magnitudes; those on the lead being the largest, and of the size represented in fig. 7. Those on the zinc were next in magnitude, as denoted in fig. 8; and the particles on the brass were still smaller, but much more numerous, as in fig. 9. The copper and tin, particularly the latter, seemed only to have had the lustre of their surfaces just dimmed, by the abundant moisture of the air. Lead, therefore, was at one extreme of the series, and tin at the other; brass holding a middle rank between the two.

This relation, however, between the particles on lead and brass, was inverted on another night, when equal squares were laid on the recently cut herbage, the particles on the brass being of the size represented in fig. 10, and those on the lead as denoted in fig. 11. As the plates of metal were the same in both cases, it is reasonable to infer that the opposite results observed were produced by the substances on which they were respectively placed. A slight trace of moisture was perceptible on the zinc, but not the least degree on the copper and tin.

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* Page 21, *Essay on Dew*, by Dr. Wells, second edition.

† Page 21, second edition.

An example of the slowness with which polished tin permits moisture to be deposited on it, occurred when a concave mirror, formed of polished block tin, was employed as an Æthrioscope, on the plan first suggested by Dr. Wollaston. The focus of the instrument, at the time the experiment was performed, was 20 inches above the ground. The night was tranquil, and dew was copiously deposited on glass, a few minutes after it was presented to the chilling influence of the transparent sky. At nine, P. M. the thermometer in the focus of the Æthrioscope indicated a temperature of 46° ; the herbage being at the same time 44° ; and the air, seven feet above the ground, 49½°. Observations, connected with some other phenomena, were made every half hour; but no trace of moisture was perceptible on the metallic surface, till two A. M., when it appeared slightly dimmed, although other substances had gained considerable increments of dew in the same time ; masses of wool, for example, having increased in weight from twelve grains to thirty. At the same moment, the focal thermometer indicated a temperature of 42½° ; that on the grass, 39° ; and that elevated in the air, 45°. In five hours, therefore, the cold of the upper sky only underwent a change of 3½° ; whereas the grass lost by radiation in the same time 5°; and the elevated stratum of air diminished its temperature 4½°. From two o'clock to three, the thermometer remained stationary, but the moisture had sensibly increased on the surface of the Æthrioscope, and increments amounting to several grains, were likewise found on other substances ; a proof, that if the general temperature remains stationary, *after* the temperature of a body is sufficiently lowered to permit the formation of dew on its surface, the farther deposition of moisture is not prevented. At four A. M. the whole metallic surface was covered with visible drops, the temperature, at the same moment (just before sunrise), indicating the maximum of cold, the focal thermometer being at 40°; that on the grass, 37°; and that elevated seven feet above the ground, 41½°. It is worthy of remark, that two plain sheets of polished tin, placed horizontally on the herbage, had not the slightest trace of moisture on them.

On another night, however, when there was every prospect of an abundant deposition of dew, the influence of the grass in promoting its formation on metals, was clearly shown. At nine P. M. two plates of polished tin, one fourteen inches by ten, and the other six by two, were laid on very short grass. Another plate of the same dimensions as the former was placed gently on the long grass. Its weight necessarily compressed the herbage on which it rested, so that the polished surface was surrounded on all sides by grass, reaching twelve inches above it. In fig. 15 the long grass is represented on two opposite sides of the tin M N, together with the compressed herbage below it. At eighteen inches above the ground, or two inches above the average height of the grass, a similar plate, O P, was placed on slender props. The temperature of the grass at the moment the plates were exposed was 60°, and of the air 65°; being a difference of 5° in the small space of three feet. At five the next morning, a great quantity of dew was formed on the grass. A register thermometer on the short herbage, indicated the maximum cold to have been 52°, and of the air, at the elevation before mentioned, 60°. The difference between these maximum depressions of temperature was, therefore, by no means considerable; and the copious deposition of dew observed was to be regarded rather as the result of the abundance of moisture in the atmosphere, than as a consequence of great difference of temperature.

The metals presented the following particulars for observation. The plates resting on the short herbage had a few scattered patches of dew on their upper surfaces, but nothing like a regular and uniform deposition. The plate M N, surrounded by the long grass, had its superior surface completely covered with minute but distinct particles of moisture; but the plate O P, elevated above the grass, was perfectly dry. This difference in the results must be regarded as arising from the different conditions, under which the plates were situated. The latter surface, it will appear, had not its temperature depressed *below* that of the stratum of air reposing on it, during the night; but the former must have been considerably colder than the column of air hovering above it. The cooling power of the grass surrounding the plate M N, and on which it also rested, must have necessarily extended its influence to the metal; and by lowering its temperature considerably, have occasioned the copious deposition observed. The upper plate not being in contact with the grass, permitted the air to pass freely on each side of it; and being itself a bad radiator, attained no condition during the night favourable to the deposition of dew. With respect to the formation of dew being less abundant on the plates resting on the short herbage, than on that surrounded with the long grass, it may, in one point of view, be regarded as a consequence of the curious fact observed by Mr. Six, that the temperature of short grass is always greater than that of long grass. The state of the herbage has always a considerable influence on the quantity of dew deposited, and the greater the body it presents, the more abundant it is likely will be the formation. That the *quantity* of herbage has a considerable effect, may be inferred from the experiment, that when one mass of wool was placed on short herbage, and another of equal size and weight on the summit of a mass of recently cut grass, fifty inches above the ground, the moisture gained by the former during the night, was only fifteen grains, whereas the increment to the latter was twenty-three.

In consequence of the plate O P having had its surface exposed to the entire canopy of the sky, but the view from the plate M N being confined to a comparatively small circular space, in the zenith of observation, it might be inferred from a principle adopted by Dr. Wells*, that the former would have gained more moisture than the latter. But the maxim of this ingenious philosopher is evidently limited to the consideration, that the bodies are in other respects similarly circumstanced. For instance, in one of the experiments instituted by Dr. Wells, to illustrate the principle in question, by bending a sheet of pasteboard into the form of the roof of a house, and placing it with its ridge uppermost, and ends open, over a mass of ten grains of wool laid on the grass; and at the same time placing another equal mass on the herbage, fully exposed to the sky, the former gained, during the night, an increment of only two grains, whereas the latter gained sixteen. In this experiment, the two masses were placed under the same circumstances, so far as contact with the grass was concerned; but in the case relative to the plates of tin, one was not only in contact with the herbage, but also surrounded by it; whereas the other was completely detached.

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* *Essay on Dew*, page 14, second edition. The principle here alluded to is the following: "Whatever diminishes the view of the sky, as seen from the exposed body, occasions the quantity of dew which is formed upon it, to be *less* than would have occurred if the exposure to the sky had been complete."

The gradual manner in which dew is deposited on the metallic side of gilded glass was pleasingly exemplified on another occasion. The parallelogram of glass was six inches by four, as represented in fig. 14. It was first exposed to the atmosphere with its metallic side uppermost, at half-past six, P. M., being about three quarters of an hour after sunset. The atmosphere was clear, and highly charged with moisture; and dew had formed on glass in a shady place, three quarters of an hour before the departure of the solar orb. A mild and gentle breeze prevailed also at the same time. No perceptible change took place in the metallic surface until eight, when minute particles of dew were visible at A, the leeward end. From the last-mentioned hour to ten, the moisture gradually increased from A to the middle part of the surface; and distinct drops were likewise deposited at D, B, E, C. As the particles increased in size round the three edges, other minute drops were successively deposited, more distant from them; and it was observed, that they accumulated with most rapidity at the leeward sides A and C. At eleven, P. M., when the sketch represented in the figure was made, an oval portion of the metallic surface was found entirely free from moisture. The same figure was also perfectly visible at midnight, when the drops at A had increased to at least an eighth of an inch in diameter; those at C being rather less. The particles at the corners D and E also preserved their superiority in size above those at B.

The difference in the appearance of dew, when deposited on tin and on glass, is sufficiently remarkable to arrest attention, not only when the moisture remains uncrystallized, but also when it is frozen. In an example that occurred of the latter case, a decrease in the magnitudes of the frozen particles could be traced from its edge to the dry and unfrozen margin surrounding a parcel of wool, placed on the middle of the plate, as represented in fig. 13: the appearance of the frozen atoms partaking, in some degree, of the lustre of the tin. The parcel of wool, in the interval from nine P. M. to midnight, gained four grains of moisture; and from the last mentioned hour, to six the next morning, thirty-two grains; thus gaining, in a double time, an eight fold quantity of moisture. The wool was frozen to the tin; and when the rays of the sun fell on the metallic surface, the crystalline particles became detached from it, and were readily collected together. The dew deposited on the glass presented an irregular fibrous appearance, its colour partaking of the greenish hue of the crystal. The icy particles on the tin were first deposited as dew, and frozen before they had collected in sufficient numbers to run into each other, and form an uniform crystalline surface. But the dew on the glass being formed at an earlier period of the night, a sufficient quantity was deposited to cause the particles to mingle with each other, and thus to present to the action of the freezing temperature a wide spread surface of water. The unequal action of the glass, combined with the law which regulates the crystallization of water, communicated to the frozen surface of dew, the fibrous and irregular character represented in fig. 12. Soon after the solar rays had impinged on the glass, filaments of ice were detached from both its surfaces, that from the upper side being much the thickest.

*Plymouth, December* 12, 1823.

# Art. II. *Description of Two New and Remarkable Fresh Water Shells: Melania setosa and Unio gigas. By* William Swainson, Esq., F.R.S., L.S., M.W.S., *&c.*

[Communicated by the Author.]

The attention of several conchologists has been excited by a new and most extraordinary fluviatile shell, belonging to the genus *Melania*, recently brought from the Mauritius. Having been favoured with its examination, I now lay before the public the following description of the shell, drawn up from the only specimen which its discoverer, Mr. Warwick, was able to procure, after diligent and often repeated searches in the same locality. I take this opportunity also of recording the characters of another freshwater shell of gigantic dimensions, equally unknown and interesting to naturalists.

## Melania. Lam: Cuv.

### Specific character.

M. testâ ovatâ, ventricosâ, spinis tubularibus seta bina porrecta basi connexa emittentibus coronatâ. Shell ovate, ventricose, coronated by tubular spines, each sheathing the base of two protruded horny bristles.

### Description.

Length, one inch two tenths, of which the spire occupies very nearly one half. In *habit* the shell resembles *Melania amarula*, (Helix amarula, Lin.) but the basal volution is more ventricose, the spire more conic, and the tip acute; it is also much thinner, and may be termed subdiaphanous; the whole shell is covered by an olive brown epidermis; the spiral volutions are angulated, and marked by from three to four transverse elevated striæ; the basal volution is without any indication of plaits, but is slightly impressed by narrow, transverse grooves, which are wide apart; these are crossed by very delicate and close-set longitudinal striæ; but whether these last are only external and belong alone to the epidermis, could not be ascertained without injury to the specimen. The most extraordinary characteristic of this shell, I shall now proceed to detail. On the upper part (or shoulder as it is sometimes called,) of the body whorl, is a row of coronated spines, perfectly tubular; these spines are very thin, and are placed parallel with and very near to the aperture; their summits are obtuse and their length variable, probably owing to some having been injured through their great delicacy; the longest measured nearly one-eighth of an inch; from the summit of each spine emerges two stiff erect acute bristles; closely adhering together, and projecting about two-tenths of an inch. The colour of these bristles is black, their surface polished, and their substance horny. They likewise possess some degree of elasticity, being easily bent by a slight pressure applied laterally; although I doubt whether they would have sustained such pressure had it been applied horizontally. These bristles it will be perceived, are completely sheathed at their base by the tubular spines, but these latter are so thin that the lower part of the bristles are distinctly seen through them; rooted, as it were, in the substance of the shell. I know not, positively, whether each spine contains *two* distinct bristles; or only one, forked or divided at about half its length, as this fact could only be ascertained by removing one of the spines, and tracing how far the division extended; but that portion which forms the lower half (and is enclosed within the spine) is so thick, as to favour the supposition of their being in pairs. These spines are continued round the middle of each volution of the spire to its apex; but they are more remote, and the bristles much shorter, than those on the body whorl; sometimes, indeed they hardly project beyond the spines. The direction of the whole is slightly incurved. The aperture is pale; and, at the top of the outer-lip, is an indented sinus similar to that seen in *M. amarula*, Lam.

*Ob.* 1. The extraordinary appearance of bristles protruding from the spines of a shell, a formation altogether unprecedented amongst this class of animals, might naturally excite, in some minds, a suspicion that it was an ingenious deception. But this idea, I think will be abandoned, when the peculiar construction of the spines are well considered. In the genus *voluta*, we have many instances of shells being crowned with thin, *vaulted* spines, but no example can be produced, of such coronated spines being *tubular;* or completely closed in their circumference, and pervious only at their summits. Now it is obvious, that this peculiar form, is of all others the best adapted to strengthen and protect the elastic bristles which they enclose: both appendages, therefore, are in unison with each other, and leave not a doubt in my mind, (setting aside the personal testimony of its discoverer) that the whole shell is in a perfectly natural state.

It is difficult to conjecture in what way the formation of the shell accords with the economy of its inhabitant. We know that testaceous mollusca, are the food of several kinds of fish, both marine and fresh-water; may not these bristles be intended by nature to defend the animal from such enemies? they would certainly be very repulsive to the lips of any fish; and in all probability would penetrate, as deep as possible into the skin. The weapons of protection or of defence with which nature has furnished different tribes of animals, are as various as they are wonderful. In the testaceous mollusca, they are confined alone to the shelly covering of the animal, who, as long as his castle is armed and entire, withdraws into its walls, secures the entrance, and remains passively secure.

*Ob.* 2. Since the above was written, Mr. Broderip informs me, another specimen of this shell has come into his possession: "carefully cleared, and every vestige of bristle removed, the hollow coronations remain."

## UNIO GIGAS.

### Specific character.

U. testâ ovato-oblongâ, depressâ, anticè alatâ et sulcis obliquis, divaricatis subradiatâ; posticè brevissimâ; dente laterali (utriusque valvæ) solitario; umbonibus brevibus, retusis.

Shell ovate-oblong, depressed anterior side winged and marked by oblique grooves in different directions; posterior side very short; lateral teeth, one in each valve; umbones small, retuse.

### DESCRIPTION.

This is truly a gigantic shell; far exceeding in size any other yet discovered as inhabiting the fresh-water, and presenting characters which leave no doubt that it has hitherto remained unknown to all conchological writers. Its extreme length is rather more than eight inches and a half; and its greatest breadth (from the ligamental to the basal margin,) five inches three-quarters.

Its form is a broad oblong-oval; obtuse at both extremities; the anterior of which is broadest and sinuated, and the posterior rounded, and so very short as to project only three quarters of an inch beyond the outer side of the cardinal teeth. The whole shell is remarkably flat but very thick in substance; and the umbones, which are unusually small have scarcely any convexity; the ligamental margin is dilated, winged, and forming in its dilation nearly two equal sides; the horny part of the ligament itself, (with the internal plate that supports it,) extends half way between the umbo and the extremity of the wing; the exterior colour of the epidermis, is dark brown, but the umbones (in this specimen) are decorticated for a considerable space around them. The sculpture of the anterior part of the shell is very peculiar; it consists of four series of short oblique grooves, or of indented wrinkles, three of which are arranged in a direction with the umbonial slope*, the other is transverse; the first of these series consists in parallel grooves which cross the wing obliquely from left to right. The next is a range of broad and sinuated indentations, wide apart and having the same inclination as the former; the third range occupies the umbonial slope, and is formed by narrow sulcated grooves, placed nearly in a horizontal direction, and diminishing in length as they approach the umbones. The fourth and last consists of several transverse grooves situated near the basal margin, and the whole presents an appearance as if the shell had been indented, in various directions, by some blunt instrument.

The inside is pearly, white, tinged with flesh-coloured purple, and stained (as is frequently the case, in fluviatile bivalves) with olivaceous yellow spots; in a perfect state of the shell the colours, probably, would be more brilliant.

The cardinal teeth are strong; deeply and irregularly striated and are obliquely transverse; in the right valve are two, and in the left valve, one; in each valve there is only one lateral tooth, a very unusual and discriminative character for the species; this

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* See Zoologiæ Illust.

GLASGOW
PHILOSOPHICAL
SOCIETY.

Fig. 1.

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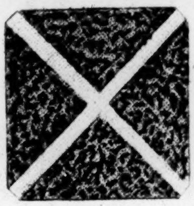

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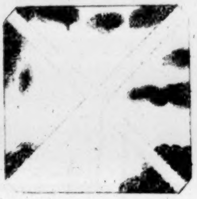

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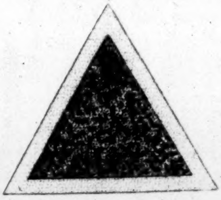

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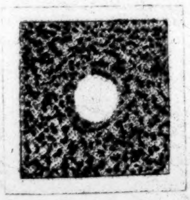

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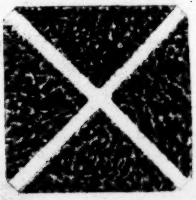

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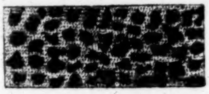

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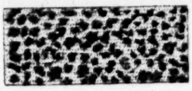

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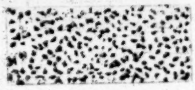

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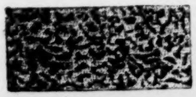

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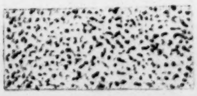

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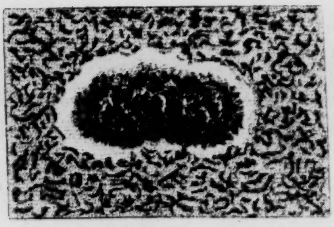

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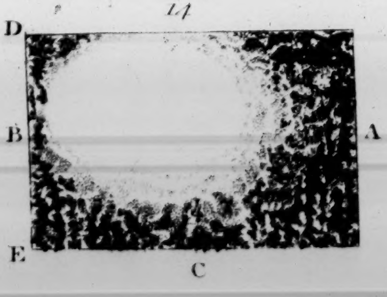

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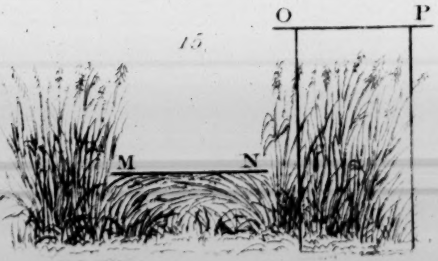

G. Harvey delin.

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Published by John Murray, Albemarle Street, London 1824.

tooth is very thick, minutely crenated, and is double the length of the ligamental plate, which latter is much elevated, broad, and terminates abruptly in a sinus extending to the point of the wing: adjoining the cardinal teeth are four deep muscular impressions, one of which is very large, and two of the others very small; the anterior impressions are slight and present nothing peculiar.

*Ob.* Two odd valves of this unique shell came into the possession of Mr. G. Humfrey, A. L. S., many years ago; and were sold with part of this gentleman's collection last spring; the shell then passed into the hands of Mr. Mawe; Mr. H. was informed it came from the river Oronokoo; this I think a very probable locality, for it has all the characteristics of an American species; its massy substance and uncommon size seems, moreover, in unison with the force and rapidity of such a vast river. I am not well satisfied as to the exact form of the dilated process on the anterior side; as in both these valves the edges had been injured and repaired: in the perfect shell this part probably may be more dilated, and may terminate in a form somewhat different from that which I have described.

*Warwick*, 1824.

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## Art. III. *On Indistinctness of Vision, caused by the presence of False Light in Optical Instruments; and on its Remedies, by* C. R. Goring, M. D.

[Communicated by the Author.]

Optical instruments in general have within the last century been brought to so high a degree of perfection, that it may almost be doubted if there remain any real improvement to be made in them; nevertheless, it has appeared to me, that in the humble department of their construction which provides against the admission of false light, there is still left some capability of a farther advancement towards perfection, which may be effected with advantage, to the performance of astronomical refracting telescopes, Newtonian reflectors, and compound microscopes.—As it is necessary to understand the nature of an evil before we can cure it, as well as to feel the utility of removing it, I shall here give a slight account of the indistinctness occasioned by *fog*, (as it is technically termed) to which I propose to apply a remedy. Thus, when we look through a telescope admitting false light at a printed bill, the plate of a clock, or other such object, especially if the day is clear, and the sun shines on it, we find, (however perfect the instrument may be in other respects,) the letters or figures do not appear nearly so black and sharp, as they will when viewed by the naked eye under the same angle, but rather of a brownish colour; in other words, the effect upon the eye, is similar to that of looking through a mist, or through glasses dimmed by moisture ; in short, what an ordinary observer would express by saying, the instrument *did not shew objects clear and distinct**. Now, on examining the pencil of rays proceeding from the eye-piece of such a telescope, with a magnifier, it will (supposing no other source of indistinctness exists,) be found surrounded by a variety of foreign rays, forming different halvos about it, instead of appearing like a spangle on a piece of black cloth, which it will do, when all the false light is stifled, as in Gregorian and Cassagrain reflectors by their eye-hole, and in refractors with erecting eye-pieces, which have a stop between the two bottom glasses, producing the same effect, by suffering nothing but the true and genuine pencil of light, from the object glass or metal, to reach the retina. Indeed, in these instruments the quantity of spurious rays would be so great, as absolutely to preclude any thing like distinct vision, without the stops and eye-holes in question. In those to which I propose to apply an equivalent contrivance for extinguishing fog, though there may not be the same imperious necessity for its application ; still I think the advantage to be gained by the improvement, is not to be despised, but will rather be admitted to be highly useful and appropriate, as placing optical instruments one step nearer perfection than they would otherwise be, by producing the maximum of distinctness and clearness of vision, of which they can be rendered susceptible, consistently with their excellence in other respects.—

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* All objects are of course equally affected by the fog, but it is more striking in those I have designated than in others.

Now, opticians have not been entirely insensible to the advantages, to be obtained by excluding all inefficient light;—being aware that no kind of blacking applied to the inside of an optical tube, is sufficient to effect that salutary purpose, they have had recourse to other means, though inadequate to the end in view.—Thus it is common in a refracting astronomical telescope, to meet with one stop and sometimes two, placed in the interval between the object and eye-glass; the apertures of the magnifiers, are likewise contracted on the same principle. But these stops are never in sufficient number, or sufficiently contracted, or placed in such situations as they should be to be efficient, at least it has never fallen to my lot to see any such. It seems to me, as if they were possessed of some superstitious dread of cutting off some of the light of the object glass by inserting stops; or perhaps have wished to shew their customers, that the apertures of their glasses were clear, it being a common trick to make a large object glass, and then to cut off the effect of the imperfect edges by a contrivance, such as has been mentioned, which ordinary purchasers are not aware of, and thus, suppose, the instrument to be much finer and better than it really is; at least it is not uncommon to meet with this species of fraud in the works of the continental artists, who are very fond of making larger object glasses than the English workmen. To enter into my subject, I shall here as succinctly as possible, describe the method which I have experimentally found to answer best for stifling fog in the astronomical refractor. It is a consideration which must obviously present itself, that if an eye-hole be placed at the end of a telescope, precisely of the size and precisely in the focus of the pencil of rays produced by any particular magnifier, that the end here proposed will be attained, as in the Gregorian and Cassagrain telescopes; it will moreover, confine the eye truly to the axis of the tube, and thus prevent us from seeing any of that colour in the image, which may always be perceived in the best instruments, when the eye is a little removed from its true position. Nevertheless, I have after sufficient trial rejected this method, as less expedient than another which I shall point out, on account of the difficulty of executing it properly with high powers, as well as that it confines the field of view, and is disagreeable to the eye. It is evident, that with high powers the pencils of rays will be exceedingly small, therefore if the aperture of the eye-hole is too large it will be ineffectual, if too small it will obstruct light: it must therefore be executed to a very great nicety, which is not always to be expected; besides in a case of such delicacy, if the eye-piece be not screwed on to a particular mark on the body of the tube, or if any of the parts of which the magnifying apparatus is composed, be more or less screwed home than at the time of adjustment, it will be highly probable, that the eye-hole will be a little thrown out of its true situation, and thus do away with the sole object for which it was constructed. Thus it is, that what is perfect in theory, will not always answer in practice. As to using eye glasses with very small apertures for the same purpose, (as contradistinguished from eye-holes, placed at the ends of the cones of light, drawn to a point by the magnifiers,) it is a method which cannot be made to exclude false rays with any degree of precision, even though their diameters are so much reduced as greatly to contract the field of view. I shall now give an account of the plan I have selected as most eligible, and which I have applied to a thirty inch and eighteen inch refractor with complete success, as it seems to me. Fig. 1. Plate II. is a drawing of the section of a refractor, in which may be seen seven stops in the course of the tube and eye-piece, (exclusive of the field bar,) five of which are placed in such a manner, and of such apertures as to pinch the cone of rays proceeding from the object glass as tight as possible, without intercepting any. It will be obvious that no foreign rays, or any that are not parallel, will be able to find their way to the eye, nor can any light be reflected from the sides of the tube, so as to become visible. To execute this, use the following method; when the telescope is finished in the usual way, and before any stop is inserted, attach its lowest astronomical eye-piece to it, and find the true solar or sidereal focus of the object glass; when thus adjusted, measure carefully with a dynameter, the size of the pencil of rays proceeding from the eye-piece, and note it down. Having then procured a plank of wood covered with paper of sufficient length, take the aperture of the object glass, and set it off at one end of the board, bisect it and draw a line at right angles to it, to the exact length of its focus: fix three strong needles into the three points of the focus and aperture of the object glass, and then stretch a fine thread over them, which will then represent the cone of light which forms the image; set off six or seven inches from the focal extremity, (an efficient stop cannot be placed nearer, without contracting the field of view,) and ascertain the distance between the threads at this point, which will give the diameter of the fifth stop. Then divide the remainder of the focal length into five equal parts, (whatever it may be,) and the distance of the threads will give the diameter of four more stops, 4, 3, 2, and 1, in the figure, all sufficiently correct for the purpose. The stops are then to be made and inserted into the tube in their proper places; it will not however be amiss to make No. 1, 2, 3, and 4, a little too large, and to confide the main business of stopping the false rays to No. 5*, which may be attached to the eye tube, and move along with it, in adjusting the focus of the magnifier. This will give the instrument the power of adjusting itself, to nearer objects on the earth, without losing any light from the effect of the stops, which otherwise must be adjusted to the shortest focus of the object glass, and supposed to act perfectly only with parallel rays. It will not be amiss to have a very small eye-hole, placed correctly in the focus of the object glass, which will give a great facility of adjusting the stops, as it will shew by merely looking through the instrument, if they are correctly placed or not. Lastly, having fixed these, apply the eye-piece, carrying the lowest magnifier as before, and again with the dynameter measure the size of the cone of rays at the eye; if it measures precisely as before, you may be quite confident you have cut off no true light*. I dare say it will be thought, there are already a very superabundant quantity of stops, but, I am sorry to say, that on examining the pencil with a magnifier, it will most probably be found, that some false light is still reflected from the eye tube, to cure which two more stops will be necessary, (Nos. 6, and 7;) both of these however, must be larger than No. 5, and No. 7 the largest of the two, or the field of view will be contracted in the low powers.—The higher the power, the nearer an efficient stop may be placed towards the focal extremity of the pencil, proceeding from the object glass; the lower it is, the farther the stop must recede, gradually of course, increasing in its aperture, (unless the length of the eye-tube is increased in proportion to the focus of the magnifier employed.) I have pitched upon six or seven inches, which is a distance for the main stop, that will suit all telescopes, and all magnifiers which are not more than one inch focus. Here it seems proper to observe, that when once the false light is duly excluded from a telescope, in the manner I have here recommended, the eye glasses may be used of any aperture, and thus the field of view may be had of any size, even with a single eye-glass, which in my opinion, when high magnifiers are used is a great convenience, as it enables us to keep a celestial object in sight more easily, though we should only see it distinctly in the axis of the telescope; moreover, should it not be thought worth while after all, to have the false light as perfectly excluded, as it possibly can be under all circumstances, the stops No. 1, 2, 3, and 4, may be rejected, and 5, 6, and 7 only executed; the consequence will only be, that some false light will be rendered sensible, when the eye is not confined to the centre of the eye-glass.

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* The way of regulating the aperture of this or any other stop to its situation in the course of the tube, will naturally be by pushing it up or down, till it strictly conforms itself to the size of the cone of rays at the point where it acts; when once it is settled for the lowest magnifier, so that the image of its aperture, and that of the object glass correctly correspond, and shew no difference in measurement by the dynameter, the business is effected for all the other powers, as they will always preserve the same relative proportion to each other, whatever may be the depth of the lens which is employed to form an image of them.

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* I think an instrument from which all the false light is utterly excluded, does not appear quite so luminous as it did before, for false rays are as capable of affecting the retina, as true ones. [If we were to turn out all the disagreeable people out of a room full of company, there would of course be fewer individuals in it, but the society would, I think, be indubitably improved by the measure.]

I have given the full complement of stops, to render the exclusion as complete as possible. As good a way as can be devised of illustrating the effect of the stops, is the following. It is known that a terrestrial telescope with an erecting eye-piece of four glasses, would be rendered nearly useless by withdrawing the little stop placed between the two glasses which erect the image, by the quantity of fog which would be let in, (supposing the instrument constructed in the usual way.) But in a telescope furnished with such stops as I have described, it is of no consequence whether the little stop is introduced or not, the performance is precisely the same in point of distinctness in both cases. In performing this experiment, it is necessary however, that the aperture of the little stop should be correctly and truly accommodated to the size of the pencil, which is formed by the glass, in the focus of which it is placed, so that it shall barely admit the image of the object glass, without cutting any of the side rays off, otherwise the experiment will not be fair.—Opticians are apt sometimes to make these apertures so small, as to intercept some of the light of the object glass as effectually as a cap over the end of it would; for the achromaticity of an erecting eye-piece depends very much upon the size of the little aperture in question. Thus where the diameter of an object glass bears a very large proportion to its focal length, it will be impossible to admit the whole of the light proceeding from it into the eye-tube, without at the same time destroying the achromatic property of the latter, by the necessity which would arise of opening this stop too wide to consist with it. In the experiment I have detailed, there is sufficient proof that the effect of my stops is equivalent, indeed more than equivalent, to that produced by the stop which is inserted in erecting eye-pieces for the purpose of procuring distinct vision. What then? is there any merit in having effected, by means of half a dozen stops, what may be done well enough by one? certainly not. But take away the erecting eye-tube, screw on an astronomical eye-piece, (either with or without a field bar,) and where I ask now, is that part of structure which is to do the work which was performed in the erecting compound magnifier, now removed, by the little stop of which I have said so much; (supposing the telescope constructed in the usual manner, that is to say, without stops in the course of the focal distance of the object glass, or at least without effectual ones?) It cannot be asserted, that there is anything equivalent in the instrument in its present condition, to the former provision in it, for the valuable purpose of excluding false rays, though the expediency and utility of it in both cases, must be equally admitted or denied, and it is clear this can only be supplied in the astronomical telescope, by some such expedients as I have resorted to.

As I conceive no one can be hardy enough to assert that there is no use in excluding false rays from a telescope intended to be used at night, for viewing the heavens, it will be superfluous for me, to set about proving that we shall see a celestial object the better, if no light, either direct or reflected, reaches the eye, save that actually proceeding from it. If the light of the heavens in a star-light night, and that of the bodies which produce it are very faint, still there is the same ratio between their brightness, and the false light they produce, (though not so conspicuous perhaps) as there is in that of terrestrial objects. Indeed it is perfectly well known to astronomers, that in the darkest night, wearing a black hood over the eyes, greatly facilitates the vision of very faint and delicate objects, such as nebulæ, &c., from the sensibility and tranquillity induced by these means in the retina, rendering it susceptible of the slightest impressions. Surely the effect of foreign light reaching the eye directly, or through the medium of a telescope, must be equally pernicious. It is in viewing the class of objects here designated, that the utility of the stops I have described will be found; no one will expect that they can render a telescope better able to define or divide a star, because these properties depend upon the perfection of an entirely different part of its structure. I shall take my leave of the subject, by asserting that if any one should choose to maintain a contrary opinion from myself on the affair of excluding false rays, he must, to preserve consistency, assert that there is no use in the eye-hole of the Gregorian and Cassagrain telescope, (if used at night,) and should therefore in using these instruments, content himself with such a one as is applied to common spy glasses, just to keep his eye in the axis of his instrument.

I now proceed to the next part of my subject, which is the consideration of the Newtonian telescope. This admirable instrument, such as could only be expected from the genius of the immortal philosopher who invented it, has, (as a necessary consequence of its construction,) among its other valuable peculiarities, that of having less false light in it than any other kind of telescope.—The same striking effect, therefore, will not be manifest in excluding the trifling fog there is in it, as in another construction where it is more abundant. Nevertheless there is something to be done.—If we examine the pencil of rays proceeding from its eye-piece with the magnifier, it does not precisely represent the image of a spangle or a piece of black cloth, as it should do;—a good deal of foreign light may be seen, formed partly by the side of the tube behind the diagonal metal, and partly by such portion of the end next the large mirror, as the plain one can reflect along with the image, together perhaps with some reverberated by the little tube which carries the magnifiers.

In Fig. II. is represented the method I have taken to remedy these imperfections in a 77-inch focus, and seven-inch aperture Newtonian*. The alterations from the common construction, are as follows: the tube which carries the magnifiers is seven inches long, instead of being only two inches or perhaps less, as is usual —the diagonal metal is likewise placed nearer than usual to the large one, so that the length of the telescope is reduced about five inches; this is of course necessary to render the pencil of rays reflected at right angles from the axis, long enough to act with the increased length of the eye-tube. (As the diameter of the spectrum of the great mirror increases as we recede from its focal extremity, more of the small plain one will in this case be called upon to act; it however will still do the work without any increase in its size; mine is $1\frac{3}{10}$ inch of circular diameter, yet its entire surface is not employed.) By this arrangement a sufficient length of eye-tube is obtained to insert the stops 5, 6, and 7, as in the refractor, No. 5, is the efficient stop as before and is $\frac{7}{20}$ inch in diameter. The extrusion of the aberrant light is complete as long as the eye is in the axis of the instrument.

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* On exhibiting these alterations to those celebrated artists, Messrs. Tulley of Islington, I learnt from them that they had lately made an arrangement of the same description, in a Newtonian telescope, made for Mr. Camfield of Northampton. These gentlemen (whose unrivalled pre-eminence in their profession, needs not my feeble testimony, or eulogium) fully admit that the exclusion of the false light, makes a great difference in the performance of the telescope, *in the day-time*, but do not seem to think any alteration is to be perceived in viewing celestial objects. I am aware, that I am paying myself an indifferent compliment in differing from such authorities. I have given my reasons for so doing, and cannot help still being of opinion, that it is scarcely possible to select any object in the heavens, and to view it without rays from a variety of others also finding their way into the telescope, and thus disturbing the singleness of vision, which would exist was there but one star or object in the heavens to emit light. I think, in particular, that the double ring of Saturn, and its belts and shadows are not perfectly seen, unless the telescope employed to view them, will show *black objects perfectly black*, and *white objects perfectly white*, and of course all the intermediate gradations of shade correctly; very few telescopes will shew the division between the rings of Saturn *quite black*, (as mine does,) owing to the false light so generally prevalent in them.

It would of course be impossible to insert any stops similar to those, 1, 2, 3, and 4, in the refractor to render the effect more complete; nevertheless, I think, were it any object, a Newtonian would by the aid of the contrivance I have applied to mine, act sufficiently well with a skeleton tube only. It now remains for me to describe the new adjustment which the adoption of so long an eye-tube has compelled me to have recourse to, for it is evident that any want of centricity and parallelism in the lenses composing the eye-glass to the axis, which might be tolerated in a very short tube, will be perfectly insupportable when aggravated by a longer one: moreover, the stop No. 5, (which I suppose to be so adjusted as barely to suffer all the light of the great metal to clear it) will, if not truly concentrical with the cone of light on which it operates, evidently impede some, as in such a case is perceptible by looking through the small eye-hole recommended in adjusting the refractor, or by examining the extreme pencil after it has passed the eye-glass with a magnifier. It is evident, I think, that the adjustment of a Newtonian is complete, when the pencil of rays which is reflected from the small metal, truly perforates the axis of the eye-tube, and the centres of the lenses composing it; it matters not, I conceive, at what angle or in what direction the said cone of rays proceeding from the large metal be thrown by the diagonal one, provided these conditions are fulfilled, (supposing of course the position of the small metal to be the centre of the tube, so that it shall truly receive the whole of the light of the great one.) We may, therefore, either adjust the small metal to the eye-tube or the eye-tube to the small metal, or we may do both, which latter will probably be the most expedient, and is the method I have preferred; I have effected it in a very simple manner, by having the tube made to fit loosely into another wider piece, which is screwed on in the usual way, to the side of the telescope:—the vacancy between them is filled up with wax, the inner tube is tight at the bottom of the external one, by the interposition of a small setting chamferred at the edge, but admits of a slight rotatory motion towards the eye-glass by heating the wax with the flame of a candle which is inserted into the external tube, and which unites them both together; time will be given to adjust it before the wax cools, when it will all set tight, and will not be liable to get out of order. Two small niches should be made, one in the shoulder of the screw of the external tube, and the other in that of the female screw to which it is applied, to be a guide that the two pieces may always be screwed home to a particular point: or it is very probable the adjustment may be spoiled, because it will be a chance if the eye-tube when fixed, is precisely at right angles to the side of the telescope. A variety of methods of effecting this adjustment will present themselves to the workman, instead of that which I have used, which though it answers very well, yet does not look very elegant or scientific. Thus, instead of the wax, three screws might be used, fixed into the external tube; or such a contrivance as is represented in Fig. II., by having counter screws to play against those by which the setting for the eye-piece is attached to the rackwork, on the side of the telescope, &c. Now it is not my intention to assert that this adjustment is absolutely indispensable, for I have not a doubt but that a superior workman might execute a long eye-tube, such as I have employed, so that nothing but the usual adjustments would be required; still I think that no Newtonian would be injured by having such an apparatus to it as I have recommended, even though the eye-tube were only of the common length; it is certain it could do no harm at least. I think I can, moreover, with confidence assert that increasing the distance between the small metal and the eye-glass, for the purpose of applying stops, will not be found to make the least sensible difference in the performance of an instrument, as far as the figure of the small metal is concerned, provided it is of the standard goodness*; if but imperfect such an alteration will evidently try it more, and this will be shewn by examining a double star which will probably vary slightly in the distance at which the stars appear separated, (cæteris paribus,) according as the eye-glass approximates to, or recedes from, an imperfect diagonal. To conclude, as an Herschelian telescope is nothing but a Newtonian, used without the interposition of a small metal reflector, whatever has been said of the latter, will equally apply to it, and the same principle in the eye-tube and adjustment, will for the same reasons be equally adapted to both, though the manner of execution will be different; I have, however, made no experiments on this kind of telescope.

[The portion of this paper relating to Microscopes is reserved for our next Number.]

## ART. IV. *The Characters of several New Shells, belonging to the Linnæan Volutæ, with a few Observations on the present State of Conchology.* By William Swainson, Esq., F.R. and L.S.

THE study of conchology has now become so general, or, if I may be allowed the term, so *fashionable*, that the number of elementary works is truly surprising. The new systems of the French conchologists have been translated, explained, and advocated, in various publications; while the admirers of the Linnæan method, have not been backward in expressing their warm attachment to the plan of the great Swedish naturalist. It is not my present intention to speculate upon the respective merits of these systems. In the study of no class of the animal kingdom have there been so few absolute facts discovered, whereon to build a truly natural system, as in that of the testaceous mollusca. In the history of those families which are known, anomalies have been discovered, which baffle explanation, and obstacles almost insurmountable, from the very nature and *habitat* of the animals, conspire to retard that rigid investigation of their economy, which must alone form the basis of their perfect arrangement.

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* It is evident that if the diagonal metal were quite perfect, it could make no difference at what distance the eye-glass was placed from it; if decidedly imperfect, it is no less plain that the nearer the eye-glass is placed to it the better, because the less of the edges will be called into action, which will of course be the worst part.

But while so many writers have been engaged in forming systems and constructing genera, the elucidation of *species* has comparatively been neglected.

An extensive acquaintance with species is the first step to a knowledge of natural divisions. In every branch of natural history those who have seen and studied the fewest individuals, will be most apt to create new genera; "when they have seen more, they will discover the intermediate links which unite different genera; and thus be forced to join what they formerly separated *". I am fearful this has not sufficiently been considered by the authors and advocates of the French systems: it may be doubtful if their generic distinctions are not too much refined; but it is certain that a knowledge of the science is daily becoming more unattainable to all but professed naturalists.

While this revolution of classification and of genera is going on, our cabinets are crowded by innumerable species, some of which we know not how to name, while others (well known by the figures of the older writers) remain undescribed: new species are continually pouring in upon us to augment the number: and although the student may be perfect in the elements of his system, he knows not how to proceed, or where to turn, if he ventures on the investigation of species.

The volumes of Lamarck (His. Nat. des Animaux sans Vertebres) have indeed done much to remedy this evil. They contain a considerable increase of new species, and a more perfect elucidation of many of the old ones; but, on the other hand, the same over-refinement which marks the characters of his genera, will be traced in the discrimination of his species; this is more particularly the case in his account of the genera Conus, Oliva, and Helix. Let me not, however, be misunderstood, as wishing to depreciate the merits of this great man. His general reputation could not be affected either by my praise or my censure. But obliged, as he has been, to employ the sight of another in finishing his latter volumes, it would perhaps have been better for his own sake, and that of the science, to which he has devoted his long life and great abilities, if they had never been published.

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* Wildenow, Principles of Botany, P. 175, Sect. 163.

The importance of monographs, or complete histories of particular tribes, or families, in every branch of natural history is unquestionably very great; for their object is, not only to ascertain the limits of genera, and the affinities and analogies, which the individuals of such genera bear to others, but likewise to include the history of all the species thereunto belonging. To accomplish this, however, is in the power of a very few. Access must be had to the rich contents of foreign museums, and of costly libraries, to supply what may be deficient in minor collections; and it is from this cause that nearly all the monographs of extensive families have proceeded from naturalists in the charge of public museums, or in the possession of immense private collections. From the labours of these men science has received the greatest assistance. But, although few can enjoy the advantages such materials afford, considerable benefits will be derived from the labours of those, who frame a correct diagnosis of individual species; particularly when relative characters are subjoined, and comparisons made between others to which they bear a resemblance. When it is considered how many rare and unknown shells have lain for years in the cabinets of mere collectors, and how much greater is the number of those species more usually seen, but which are likewise unrecorded, the value of these isolated descriptions will be rightly understood. They are the indispensable materials for completing a general survey of the natural world, and constitute the ultimate object of all systems; namely, such a knowledge of the individuals, as will enable the student to assign to each "a local habitation and a name."

I shall now proceed to describe several beautiful shells; mostly of uncommon rarity, and apparently unknown to modern writers: the four first belong to the genus *Voluta*, as it is now restricted; and the remainder to *Mitra*, a genus to which I have long paid much attention, with the ultimate hope of illustrating it by a distinct monograph.

### VOLUTA. Lam. (Div. 1. Musicales.)

Voluta *chrysostoma*.

V. testâ ovatâ, albente, lineis angulatis maculisque castaneis ornatâ; anfractibus spinis brevibus, concavis coronatis; apice crasso, obtuso, lævi; aperturâ aureâ.

Shell ovate, whitish, with angulated chestnut lines and spots; whorls crowned by short concave spines; apex thick, obtuse, smooth; aperture golden.

Voluta chrysostoma. Sec. Exotic Conch. Fas. 5. ined.

Voluta luteostoma? testa obovata, angulata, lineis et venis fuscentibus in fundo albido undulata sub-perforata, anfractibus cinctis nodis conicis, apice obtuso, basi valde emarginata, columella plicata plicis quatuor solidis, fauce lutea. Chemnitz xi. p. 18. tab. 177. F. 1707-8.

### DESCRIPTION.

The shell in its habit, approaches V. *vespertilio*: its total length is about two inches, of which the spire occupies not more than half an inch: its form is oval, and its surface without sculpture: the basal volution, and the two first whorls of the spire, are crowned by a row of short thin vaulted spines, rather acute, and resembling those on V. *diadema*, (Ex. Conch. Fas. 1.) The remaining three spiral whorls are perfectly smooth, the middle one being by much the largest, and the whole forming a thick and somewhat obtuse cone. The base is deeply emarginate, and the plaits on the columella, (which are four in number) are very thick. The ground colour of the specimens before me is nearly white, with broad longitudinal shades of deep chestnut, broken into rows of angulated whitish spots of various sizes, and disposed in a longitudinal direction. The spire is white, with a few brown undulated lines on the lower whorls; the inner lip yellowish white, and the throat or inner aperture golden yellow.

This shell, as far as regards English collections is unique. It is now in the possession of Mr. Mawe, I believe, and in all probability may be found to inhabit the Indian Ocean.

The *Vol. luteostoma* of Chemnitz,(a shell passed over by Lamarck, and all systematic writers,) bears a strong resemblance to this species, but I have many doubts if it be really the same. In this genus, the form and sculpture of the terminal whorls of the spire, afford the most certain specific distinctions; now in the V. *luteostoma*, these terminal whorls are represented as graduating to an obtuse point, whereas in the shell above described, they are very thick and papillary. V. *luteostoma* is stated to be "*subperforata*," but this V. *chrysostoma* bears not the slightest indications of such a character, neither is the description, "anfractibus cinctis *nodis conicis*," applicable, if intended for the latter species. On the other hand, the two shells agree in their general form, habit, the golden colour of their apertures, and nearly the pattern of their markings. I have preferred however, for the present, to keep them distinct; because every conchologist must be sensible, more perplexity has been introduced into the science, by creating too few species than too many.

### VOLUTA GRACILUS.

V. testâ oblongo-fusiformi, lineis undulatis pictâ; spirâ producta plicatâ; labio exteriore subreflexi; columellâ 4 plicatâ.

Shell oblong-fusiform, with undulated lines; spire lengthened, plaited; outer lip sub-reflected; pillar 4 plaited.

### DESCRIPTION.

This is a most elegant shell, belonging to the same group as that filled by V. *undulata* and its allies; from all of which it may at once be known by the great prolongation of its spire, which is nearly the length of its aperture. The whole shell does not exceed two inches and a half in extreme length; the basal volution is smooth; but the three next whorls of the spire are plaited, and slightly nodulous; these plaits then disappear, and leave the terminal volutions quite smooth; the apex is obtuse, but not enlarged; the base of the shell is contracted, and the emarginate notch rather slight; the margin of the outer lip is somewhat reflected, and on the columella are four slender and nearly equal plaits. The colour is pale brown, elegantly marked by longitudinal, slender, waved, and angulated lines of a deep fulvous brown; at the top, bottom, and middle of the basal whorl, these lines are more thickened and deeper coloured, so as to form three transverse bands. Another specimen of this species was covered over with a reddish tinge, which nearly obscured its markings. The brown lines are also continued on the spire, but are fewer and more remote.

*Ob.* Two specimens of this elegant voluta were brought home by one of the South Sea trading vessels from the Bay of Island, they are now in the possession of Mr. Mawe.

### VOLUTA COSTATA.

V. testâ ovato-oblongâ, costis sub-mucronatis, pallidâ, lineis fulvis interruptis fasciatâ; basi granosâ; spira mediocris apice lævi, obtuso; columellâ multiplicatâ, plicis tribus inferioribus maximis.

Shell ovate-oblong, with sub-mucronate ribs, pale, and banded with interrupted lines of fulvous; base granulated, spire moderate, the tip smooth and obtuse; pillar many plaited, the three inferior plaits largest.

The situation of this species, appears intermediate between the *Vol.* festiva and mitræformis of Lamarck, but its form cannot be compared to any other. It is little more than two inches and a quarter in extreme length; the spire is rather produced, and occupies one inch. With the exception of the terminal whorl at the apex of the spire, (which is perfectly smooth and obtuse,) the whole shell is marked by numerous, regular, convex ribs, about the same thickness as the breadth of the space which occurs between them; these ribs form a row of short obtuse spines, which crown the summit of each volution; leaving between them and the suture an open channel; at the base of the shell are deep striæ, which cross the ribs, and produce a rough granulated surface. The ground of the shell is pale flesh colour, crossed on the ribs by bands of short slender fulvous lines; interspersed by a few orange spots: the aperture is also flesh-coloured, and the margin of the outer lip sharp and rather inflected. The upper part of the columella is crossed by numerous slender plaits, and at the lower part are three others much larger.

Described from a specimen in the possession of Mr. Mawe.

*Ob.* 1. The V. festiva of Lamarck, except from his description, is a species unknown to me. According to the writer, it differs from V. *costata* in being fusiform and ventricose, resembling in shape V. magellanica; it likewise appears destitute of the numerous small plaits on the columella, and of the obtuse coronations, formed by the summit of the ribs. These two shells with *V. nucleus* Lam. (which is Voluta harpa of Mawe's Introd.) V. *mitræformis*, and another undescribed species in my possession, constitute a new group in the genus; characterized by having the principal plaits on the pillar, situated at the base of the aperture.

*Ob.* 2. It may be necessary to observe, that although two shells already appear in the Linnæan classification under the name of *voluta costata*, neither of them in fact, belong to this genus as it now stands. One of these (the voluta nassa of Gmelin,) is a young shell of a species of *Nassa* Lam.; the other, (Vol. costata of Gmelin and Dillwyn,) is the Mitra Subulata of Lamarck.

### MITRA. Lam. Cuv.

### MITRA tessellata.

M. testâ ovatâ, lævi, striis transversis remotis et punctis, albente, lineis fulvis transversis et longitudinalibus cancellatâ, labii interioris basi fuscâ; labio exteriore lævi.

Shell ovate, smooth, with remote transverse punctured striæ; whitish, cancellated by transverse and longitudinal fulvous lines; inner lip brown at the base, outer lip smooth.

### DESCRIPTION.

Habit of Mitra pertusa, but is much smaller in size, and less ventricose; the spire also is shorter in proportion, but more thickened and obtuse; total length one inch three quarters; the whole shell is crossed by delicate remote striæ, which are minutely punctured; the aperture is rather longer than the spire, and together with the inner lip is pure white, the base of the latter is however stained by dark chestnut brown; the outer lip is rather inflected and is perfectly smooth; this latter character will at once distinguish this species from all its allies; the pillar has four plaits. The colour is uniform yellowish white, with slender fulvous, transverse lines following the indented striæ; these fulvous lines are crossed by others more broken and produce a singular resemblance to the mortar divisions of brick work; adjoining the suture is a row of small fulvous spots.

This shell I only know from a beautiful specimen in the possession of Mr. Mawe.

### MITRA GUTTATA.

M. testâ ovatâ, sub-fusiformi, lævi, striis transversis punctis, fulvâ maculis albis variâ; labio exteriore crenato; columellâ 5-plicatâ.

Shell ovate, sub-fusiform, smooth with transverse punctured striæ, fulvous variegated with irregular white spots; outer lip crenated, pillar 5-plaited.

### DESCRIPTION.

Habit of the last; but is a much smaller shell, having the base more contracted, and the tip more acute; the striæ also are deeper and the punctures larger; it seldom exceeds an inch in length; the whole shell is brownish yellow, irregularly marked by white spots and blotches, these last spread over the spire, and form an irregular band across the middle of the body whorl; the aperture is white, the outer lip crenated, and the pillar has five plaits.

*Ob.* Two specimens of this species are in my own collection; but I am unacquainted with its locality.

### MITRA FUSCA.

M. testâ crassâ, ovatâ, lævi, fuscâ; spiræ contractæ suturà subtilissimè crenatâ; labio exteriore crasso, gibbo, lævi; columella 4-plicatâ.

Shell thick, ovate, smooth, brown; spire contracted, suture minutely crenated; outer lip thick, gibbous, smooth; pillar 4-plaited.

### DESCRIPTION.

Length one inch, habit of M. *crassata* Sw. the basal volution is thick and rather ventricose; the spire is short and abruptly slender, having the upper margin of the whorls projecting beyond the suture, and minutely but regularly crenated; the whorls are likewise crossed by a few remote striæ, the inner margin of the lip is thickened, and gibbous, except at the aperture, which is white.

The whole shell is of a uniform fulvous brown; inhabits the Indian ocean, and is very rare.

### MITRA ACUMINATA.

M. testâ crassâ, lævi, albente; spirâ contractâ, attenuatâ acuminatâ, aperturâ longiore; labii exterioris crassi margine inflexo, lævis; columellâ 4-plicata. Shell thick, smooth, whitish; spire contracted, attenuated, acute, longer than the aperture; outer lip thick, the margin inflexed and smooth; pillar 4-plaited.

On a cursory glance this shell (if in an imperfect state) might easily be mistaken for the last; when perfect, it is about an inch and a half long (the spire occupying considerably more than half this length) and is faintly striated. The spire when uninjured is long, abruptly contracted, and terminates in an acute point. This part is so delicate, that in two specimens out of three which have come under my notice, it was wanting. The whorls are crossed by delicate indented striæ, but are not in the least convex; this character gives the suture an appearance of being channelled; the outer lip is somewhat inflexed, the margin smooth, thick and slightly gibbous within the rim; the base is obtuse, and the whole shell white, covered with either a yellow or reddish brown epidermis.

Inhabits the Mauritius.—A small though beautifully perfect specimen is in the possession of Mr. Mawe.

### MITRA CARINATA.

M. testâ gracili, fusiformi, fuscâ, anfractibus medio carinatis, juxta suturam striatis, columellâ, 4-plicatâ.

Shell slender, fusiform, brown, whorls carinated in the middle, and striated transversely near the suture; pillar 4-plaited.

### DESCRIPTION.

A remarkably slender fusiform shell, about an inch long; the spire being of equal length with the aperture; the shoulder of the basal volution, and the middle of the spiral whorls are crossed by a carinated ridge; between which and the suture, are two or three elevated transverse striæ; the rest of the shell is quite smooth; the aperture is white, and smooth within; the inner lip marginated, and the pillar 4-plaited. It is covered by a uniform brown epidermis, beneath which the colour is yellowish; base deeply emarginate, and slightly recurved.

Inhabits Sierra Leone, from whence it was received by Mr. Mawe.

It is a species at once distinguished by its crenated whorls, and should be placed in the same division as *M. vulpecula and melongena.*

### MITRA STRIGATA.

M. testâ lævi, castaneâ, strigis longitudinalibus obsoletis, albentibus ornatâ; aperturâ spirâ breviore, alba; columellâ 4-plicatâ.

Shell smooth, chestnut, with obsolete longitudinal whitish stripes; aperture white, shorter than the spire; pillar 4-plaited.

Habit of M. *carbonaria*, Sw. and M. *melaniana*, Lam: the specimen before us measures two inches in length, the spire occupies an inch and one-tenth and is rather thick. The top of each whorl where it joins the suture, is turned and prominent, every part of the shell is destitute of sculpture and very smooth; the base is contracted and the pillar has four teeth, with the indication of a fifth. The colour is a rich glossy chestnut, striped at unequal distances, with paler, narrow, longitudinal stripes, which form dots of pure white adjoining the suture; the aperture and inner lip are also white.

The only specimen of this beautiful mitre with which I am acquainted, is in the possession of Mr. Mawe.

### MITRA BICOLOR.

M. testâ lævi, fusiformi, albâ, faciâ fuscâ latâ cinctâ, spirâ anfractusque vasalis parte superiore striis cancellatis punctis insculptis; striis basalibus simplicibus.

Shell smooth fusiform, white with a brown band; spire an upper part of the body whorl with cancellated punctured striæ; base with simple striæ.

### DESCRIPTION.

Shell about three quarters of an inch long, in shape, habit, and even in colour, resembling M. *casta* (Zool. Ill. pl. 48.) but the brown band, (which in that shell is merely formed by an external epidermis,) in this is internal, and delicately waved with capillary longitudinal lines of whitish; the longitudinal striæ are clouded and simple, but the transverse striæ are more remote, and deeply punctured; those in the middle of the body whorl, and of the base are likewise simple; the plaits on the columella are four, and very prominent; the base of the pillar is tipt with brown.

*Ob.* This shell, together with M. *casta, olivaria, dactylus* and *olivæformis,* constitute a particular group, distinguished by the plaits of the pillar extending far beyond the aperture.

Inhabits the South Seas? mus. nost.

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## ART. V.—*Account of the Earthquake in Chili, in November,* 1822, *from Observations made by several Englishmen residing in that Country.*

[Communicated by F. PLACE, Esq.]

CHILI is a long narrow country, lying between the mountains of the Andes on the east, and the Pacific Ocean on the west. It extends from 20° 20′ to 43° 50′ south latitude, and from 68° 50′ to 74° 20′ west longitude from Greenwich, its length being about 1350 miles, and its average breadth about 130 miles.

While under the dominion of Spain, Chili was visited by very few Europeans. Its great fertility, its abundance of metals and minerals, its agreeable and healthy climate, have, since it has been declared independent, induced a considerable number of Englishmen, and a few other foreigners, to become residents, and the number is continually increasing.

The country rises gradually but irregularly from the-sea coast to the mountains; it is exceedingly diversified, but the principal feature is its formation into valleys, surrounded by hills, many of them rising to a considerable elevation.

The whole country may be divided into two regions or climates, the one humid, the other dry, separated from each other by the river Maule, which in 35° 10′, falls into the Pacific Ocean.

South of the river Maule the climate is variable; rain falls at intervals during the whole year, and timber trees are in abundance. North of the river Maule the rains are periodical, and fall only during a particular time of the year. At Valparaiso, the principal sea-port of Chili, and for about forty miles to the northward, the rainy season commences in May and terminates in September. Further to the northward, the rainy season is of shorter duration, diminishing gradually, until at the northern extremity of the country, it totally ceases. To the southward of the Maule the time in which rain falls gradually increases, and, at the southern extremity of the country, there are but few intervals of dry weather.

Chili is never free from earthquakes; scarcely a week ever passes without one or more being felt, in some part of the country, but as the shocks seldom do any damage, the inhabitants pay but little regard to them.

It is now nearly a hundred years since the former great earthquake, and a persuasion seems to have prevailed among the people that no very considerable earthquake would happen oftener than once in two hundred years. Partial earthquakes, doing much damage, have always happened at intervals of a few years. The town of Coquimbo was nearly destroyed by an earthquake in 1820. The shock, was local, and produced no alarm in other parts of the country.

On the fourth of November, 1822, the town of Copiapo, in S. lat. 27° 10′, was visited by a severe shock, which damaged many houses; this was followed, the next day, by a much more violent earthquake, which nearly destroyed the town, and did considerable injury to the town of Coquimbo, in S. lat. 29° 50′.

The great earthquake on the night of the 19th of November, 1822, was felt over the whole surface of the country, from the mountains to the sea, and from one extremity to the other. Its force seems to have diminished in a pretty exact proportion to its distance from Valparaiso.

Its effects are thus described by an Englishman, residing at Concon, near the mouth of the river named in the maps "Rio Quillota." Concon is about fifteen miles N.N.E. of Valparaiso, as the crow flies.

"At half-past ten, on the night of the 19th November, I felt the first oscillation. I was writing at the time; starting from my chair, I paused for an instant, expecting the shock would subside, as others had done; but the falling of glasses from the sideboard, the cracking of the timbers, and the rattling of the tiles from the roof, fully apprized the whole family of their danger, and all ran out of the house. The house was violently agitated, and was falling to pieces, but freed from the apprehension of being buried in the ruins, my attention was forcibly drawn to the phenomena, which I endeavoured to observe as accurately as possible. Scarcely, however, was this resolution taken, and before the first shock had entirely subsided, a second and much more violent one succeeded; this was accompanied by noise, which appeared to be deep seated in the earth, perpendicularly to the spot on which we stood. The duration of this shock was about two minutes; it was succeeded by a third, also accompanied by noise, less loud than that which accompanied the preceding shock. The shock was less violent than either of the two former shocks, and of less duration. These shocks occupied about five minutes of time. Shocks, at intervals, of four and five minutes, continued for nearly an hour, after which, they became less frequent during the remainder of the night, and of very different intensities, some being rather severe, and others hardly perceptible. The three principal shocks may be said to constitute the earthquake.

"At the commencement of the earthquake, the atmosphere was, as is usual at this time of the year in this country, quite free from clouds, the moon and stars shone with splendour; there was no atmospheric indication of change of any sort, either before or after the earthquake. Some persons say they saw an unusual light in the horizon to the southward, but I, who was expecting some change, and was prepared to observe any that might have occurred, saw none whatever.

"During the earthquake the ground rose and fell with great violence, and with almost inconceiveable rapidity. There was certainly no undulatory motion, though many unobserving and unreflecting persons suppose this to have been the case. I had a strong suspicion at the time, since confirmed by observation of its effects, that there was a powerful horizontal motion, but as I could not perceive it as coming from any particular point, I concluded at the time that I was mistaken. The circumstances which make me now conclude there was a horizontal motion, are observations I have since made in many places, in which walls, and even houses, have been partially twisted round, and from the fissures round the roots of the largest trees. At Quintero, ten miles to the northward of Concon, are several large palm-trees; three of these standing so as to form an equilateral triangle, lashed one another like willow rods, and beat or shook off many of their branches. The motion of these trees seems to have been horizontal and circular, since each of them cleared a space in the ground round its stem, several inches wide, and this was the case also with other large trees in different places.

"The sensation we experienced during the earthquake, was probably the same we should have felt had we been conscious that a mine had been sprung beneath us, and was about to blow us all into the air.

"On examination next morning, at daylight, I found the earth full of fissures, some of them very small, while others were from two to three feet wide. In many places sand had been forced up, and had formed small hillocks. In the most recently formed alluvial soil near the river, water and sand had been forced up together, there being many large truncated cones of clean washed sand, each of which had a hollow in the centre, like the crater of a volcano. The same phenomenon was observed in several places; in other places, large quantities of soft mud had been forced up, and spread itself over the surface of the land.

"The surface of the country has been raised all along the coast, as far as my information extends. It seems to have been raised highest at the distance of from two to three miles from the shore, diminishing both ways. The rise on the coast is from two to four feet; at the distance of a mile inland, the rise must have been from five to six or seven feet; for in the cut for the tail water course of a mill, at the distance of about a mile from the sea, a fall of fourteen inches has been gained in little more than a hundred yards.

"At Valparaiso, near the mouth of the Concon, and along the coast northward to Quintero, rocks have appeared in many places, where none before were visible. The high-water mark along shore is about three feet above the place the tide now reaches, and a vessel, which had been wrecked on this coast, and which could only be approached at low water in a boat, is now accessible on dry land at half tide *.

"At Valparaiso, not a single house escaped being damaged; it is somewhat remarkable, however, that although the ground was raised bodily, and considerably, those houses whose foundations were on the rocks, were less damaged than those built on the alluvial soil. All the houses at Valparaiso are built of adobes (sun-dried bricks), cemented with clay. These were thrown into heaps of rubbish, or torn and rent in all directions. The town had the appearance of having suffered a heavy and long continued bombardment. Upwards of three hundred persons were buried in the ruins. Had the earthquake happened two hours later, very few of the inhabitants would have escaped.

"After the earthquake, the inhabitants encamped upon the hills above the scene of desolation, in the best way they could; this was thought less of a hardship than it would have been thought in most other countries, from the fine warm weather, the certainty as was concluded of dry weather, and the small quantity of dew which, at this season of the year, falls in Chili. To these hills goods of all sorts, furniture, and every thing else, were brought, and laid in the open air. The damage done to this thriving town will not be repaired in many years.

"The church of La Merced presented a striking instance of the

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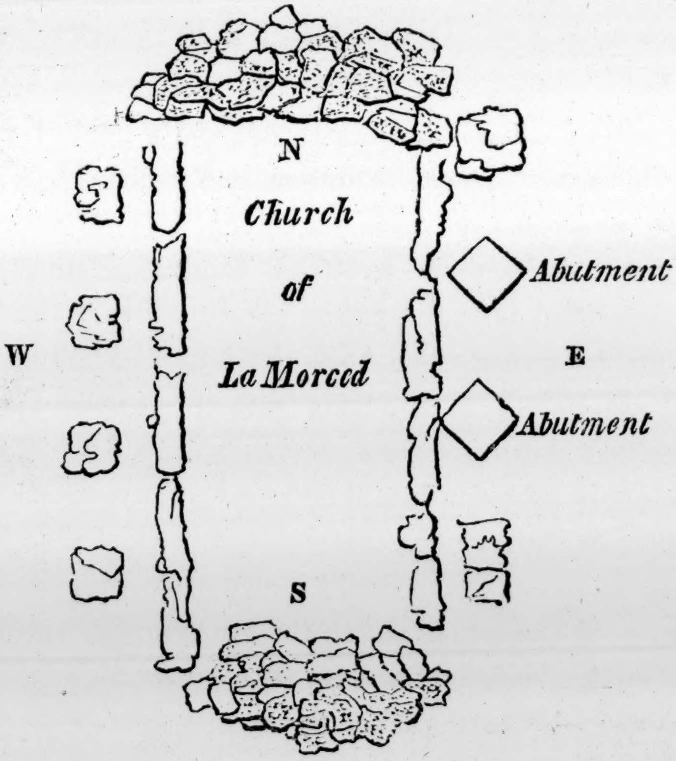
* There is very little variation of tides on this coast, the sea never rises more than four feet at the full of the moon.

violence of the earthquake; the tower, sixty feet high, which served as a belfry, was levelled to the earth. Its solid walls of burnt bricks, well laid in mortar, were shivered in pieces; the two side walls, full of rents, were still standing, supporting part of the shattered roof, but the two end walls were entirely demolished. On each side of the church were four massive abutments, six feet square, of good brick work; those on the western side were thrown down, and broken to pieces, as were two on the eastern side; the other two were twisted off from the wall, in a north-easterly direction, and left standing.



"On board the admiral's ship in the harbour, where more secure than ashore, the effects of the earthquake, so far as the situation permitted, observations were made with great accuracy. Here three distinct shocks were felt, the second was observed to be by far the strongest, and its duration, as had been noticed at Concon, is stated at two minutes. The effect upon the ship was the same as would have been produced had she suddenly sunk down upon a rock. It appeared as if her bottom had been struck with prodigious force;

the vessel vibrated in an extraordinary manner, her timbers cracked, and she appeared strained throughout.

"At Santiago, the capital, at ninety miles distance from the sea, and about twenty miles from the mountains of the Andes, the earthquake was less severe; no houses were thrown down, although many, as well as the churches, were much damaged, but no lives were lost. Here, however, as in other places, the inhabitants removed from the town, and camped out in the open air. The effect of the earthquake at Aconcagua, about fifty miles N.N.W. of Santiago, was much the same as at Santiago. Millipilla, sixty miles S.E. of Valparaiso, suffered less than either Santiago or Aconcagua; but, at Casa Blanca, not a single house or wall of any kind was left standing. At Mapel, the shocks were very severe, great part of the village was destroyed, and a pool of water was formed in the market-place. Quillota also suffered to a considerable extent, many houses were destroyed, and all were more or less damaged. At Valdivia, in 39° 50′ S. lat., one shock only was felt; it is described as having been "pretty sharp," but it did no damage. At the moment the shock was felt, two volcanoes in the neighbourhood burst out suddenly with great noise, illuminated the heavens and the surrounding country for a few seconds, and then as suddenly subsided into their usual quiescent state.

"Although no atmospheric changes appeared at the time of the earthquake, there can be no doubt that very considerable changes took place. The weather, after the earthquake, continued as usual; but on the evening of the twenty-seventh of November, just eight days after the earthquake, the country, for a great extent, was visited by a tremendous storm of rain, accompanied with heavy gusts of wind; the rain continued all night, producing terror and dismay among the people. Every thing saved from the earthquake, was exposed in the open air, or under such temporary coverings as could be constructed with the few materials time and circumstances permitted. Few of the tents, under which part of the people lived, were water proof. Many were living in enclosures called ramadas, made of dried boughs and bushes, open to the heavens, and many had no other fence than could be formed of their furniture or other effects. Rain towards the close of the month of November had been expected by no one, and no preparation to defend either persons or property from its effects, had been made. Rain had never before fallen in the country, even at a small distance north of the river Maule, in the month of November. The consequences anticipated from the rain, which, from appearances, was likely to continue, were of the most distressing nature. The total destruction of the houses which had been injured, as well as that of the goods, merchandize, and furniture, which had been collected, and of the growing crops, was anticipated by all. Its immediate effects, had it continued, would have been intermitting and malignant fevers. These apprehensions caused the people to pass a night of indescribable agony. The rain, however, ceased suddenly towards morning, and the weather became settled as usual.

"The greatest force of the earthquake appears to have been felt at the distance of about fifteen miles N.E. of Valparaiso; the whole country, from the foot of the Andes to far out at sea, has been raised; the rise has, however, been very unequal.

"As the earthquake was felt at Copiapo in the north, and at Valdivia in the south, its extent, from north to south, exceeded nine hundred miles. Where the shocks were most severe, the earth has been raised the highest, and its not subsiding again to its former level has probably been occasioned by the innumerable fissures and multitude of small cracks caused by the repeated explosions, by which the density of the whole mass has been somewhat altered.

"Many persons to the northward of Valparaiso thought the direction of the shocks was from the south-west, while those to the southward thought they came from the north-west. If the principal force, as it appears to have been, was exerted within a circle of about fifty miles diameter, the centre of which was a little to the N.E. of Valparaiso, the direction of the shocks might have been, as those at a distance, to the north and south of that space, have described them. Most persons who live near the coast, suppose the shocks to have come from seaward, either to the northward or southward, as had been mentioned, while those who resided within the circle described, conclude they were produced by explosions, perpendicular to the earth's surface. It does not appear that the earthquake extended into the mountains of the Andes; no change whatever was observed in any of these mountains, except as has been related near Valdivia, and here the volcanic ridge is nearer to the sea and less elevated than in any other part of Chili. The surface over which, or rather under which, the earthquake extended ashore, cannot be less than 100,000 square miles.

"During the earthquake the sea, for a considerable distance along the coast, receded and returned several times. At Quintero, the fishermen who live upon the beach, fled in terror to the sand-hills. At Valparaiso, a man-of-war's boat, going ashore, landed at the door of the Custom-house, which is twelve feet above the usual high-water mark. Neither the recussion, nor the retrocession of the sea, were as violent as might have been expected.

"Up to the end of September, 1823, the date of the last accounts, earthquakes continued to be felt; forty-eight hours seldom passed without a shock, and sometimes two or three were felt during twenty-four hours."

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## ART. VI. *On Evaporation.* *By* J. Frederic Daniell, Esq., F.R.S., M.R.I., *&c.*

[Communicated by the Author.]

THE subject of evaporation has occupied, at various times, much of the attention of natural philosophers, and many accurate and interesting observations have been recorded of the formation and diffusion of elastic fluids, from various kinds of liquids. The circumstances, especially, attending the rise and precipitation of aqueous steam in the atmosphere, are acknowledged to be important in the highest degree, as upon their silent influence depends the adjustment of those important meteorological phenomena, with which is connected the welfare of all the organized creation. The labours of De Luc, De Saussure, and particularly of Mr. Dalton, have thrown considerable light upon this never-ceasing process; but something appears to be still wanting to complete the investigation, and to follow up the results to their ultimate consequences. The following observations, however inadequate to fulfil this desirable purpose, may possibly attract some attention to the subject, and may be the means of indicating the points which most require elucidation.

It is a well-known fact that water, under all circumstances, is endued with the power of emitting vapour, of an elastic force proportioned to its temperature. It is also well understood, that the gaseous atmosphere of the earth, in some degree, opposes the diffusion, and retards the formation of this vapour; not, as Mr. Dalton has shewn, by its weight or pressure, but by its *vis inertiæ.* What is the amount of this opposition, and by what progression it is connected with the varying circumstances of density and elasticity, have never yet been experimentally explained.

It may facilitate the comprehension of the subject, to distinguish three cases with regard to the evaporating fluid: the first, when its temperature is such as to give rise to vapour equivalent in elasticity to the gaseous medium, and when it is said to boil; the second, when the temperature is above that of the surrounding air, but below the boiling point; and the third, when the temperature is below that of the atmosphere.

With regard to the first, all the phenomena have been accurately appreciated. The quantity evaporated from any surface, under any given pressure, is governed, in some measure, by the intensity of the source of heat, and is in no way affected by the motions of the aërial fluid. The elasticity of the vapour is exactly equivalent to that of the air, which yields *en masse* to its lightest impulse. When disengaged, it is immediately precipitated in the form of cloud, giving out its latent caloric to the ambient medium; and under that form is again exposed to the process of evaporation, according to the laws of the third division of the process. All the phenomena attending the process of boiling, have been ably investigated by Gay-Lussac, Dalton, Ure, and Arch-deacon Wollaston; but, as they have but little connexion with the atmospheric relations, which are the particular object of the present paper, I shall proceed to the second case of evaporation.

When the evaporating fluid is of a higher temperature than the surrounding air, but not so high as to emit vapour of equal elasticity to it, the exhalation is proportionate to the difference of temperature. The gaseous fluid, in contact with the surface, becomes lighter by the abstraction of portions of the excess of heat, and, rising up, carries with it, in its ascent, the entangled steam. This, as in the former case, is precipitated, and, in the form of cloud, exposed to the third species of evaporation. This process is not only proportioned to the difference of temperature, and the elasticity of the vapour, but is also governed by the motion of the air. A current or wind tends to keep up that inequality of heat upon which it depends, and prevents that equalization which would gradually take place in a stagnant air. Such is the evaporation which often takes place in this climate, in Autumn, from rivers, lakes, and sea, and which is indicated by the fogs and mists which hang over their surfaces.

It is, however, the third modification of circumstances, which is the most interesting in the point of view which I have suggested, and from which I have merely distinguished the preceding, to free the subject from ambiguity. When the temperature of water is below that of the atmosphere, it still exhales steam from its surface; but, in this case, the vapour, neither having the force necessary to displace the gaseous fluid, nor heat enough to cause a circulation, which would raise it in its course, is obliged to filter its way slowly through its interstices; and the nature of the resistance it meets with in this course is the first object of investigation.

The force of vapour, at different temperatures, has been determined with great accuracy, and the amount of evaporation has been shewn to be *cæteris paribus*, always in direct proportion to this force. The quantity is also known to depend upon the atmospheric pressure, but I know of no experiments which establish the exact relation between the two powers. I attempted to elucidate the point as follows:—

By enclosing in a glass receiver, upon the plate of an air-pump, a vessel with sulphuric acid, and another with water, and by properly adjusting the surfaces of the two, it is easy to maintain, in the included atmosphere of permanently-elastic fluid, an atmosphere of vapour of any required force; or, in the usual mode of expressing the same fact, the air may be kept at any required degree of dryness. The density of the air, in such an arrangement, may, of course, be varied and measured at pleasure. Now there are three methods of estimating the progress of evaporation in such an atmosphere: the first, and most direct, is to weigh the loss sustained by the water in a given time; the second, to measure, by a thermometer, the depression of temperature of an evaporating surface; and the third, to ascertain the dew point, by means of the hygrometer.

### *Experiment* 1.

The receiver, which I made use of, was of large capacity, and fitted with a hygrometer. I placed under it a flat glass dish, of 7½ inches diameter, the bottom of which I covered with strong sulphuric acid. The glass bell but just passed over it, so that the base of the included column of air rested everywhere upon the acid. In the centre of the dish, was a stand with glass feet, which supported a light glass vessel of 2·7 inches diameter, and 1·3 inches depth. Water to the height of an inch was poured into the latter, the surface of which stood just three inches above that of the acid. A very delicate thermometer rested in the water, upon the bottom of the glass, and another was suspended in the air. It may be necessary to observe, that the sides of the vessel were perpendicular to its bottom, which was perfectly flat. The height of the barometer was 29·6, and the temperature of the water 56°. In twenty minutes from the beginning of the experiment, the hygrometer was examined, and no deposition of moisture was obtained at 26°.

This being the greatest degree of cold which could be conveniently produced by the affusion of ether, the experiment was repeated, with a contrivance which admitted of the application of a mixture of pounded ice and muriate of lime, to the exterior ball of the hygrometer. In this manner the interior ball was cooled to 0°, without the appearance of any dew. The temperature of the water and air were, in this instance, 58°, and the pressure of the atmosphere 30·5.

From this experiment it appears, that in the arrangement above described, the surface of water was not adequate to maintain an atmosphere of the small elasticity of ·068 inch; in other words, the degree of moisture in the interior of the receiver could not have exceeded 129, the point of saturation being reckoned 1000. How much less it was than this, or whether steam of any less degree of elasticity existed, the experiment, of course, did not determine. We may reckon, however, without any danger of error in our reasoning, that the sulphuric acid, under these circumstances, maintained the air in a state of almost perfect dryness.

### *Experiment* 2.

The same trial was made with atmospheres variously rarefied, by means of the pump. No deposition of moisture was, in any case, perceived with the utmost depression of temperature, which it was possible to produce; and the state of dryness was as great, in the most highly attenuated air as it was in the most dense. In the higher degrees of rarefaction, the water however became frozen.

### *Experiment* 3.

The water, which had been previously exposed to the vacuum of the pump to free it from any air in solution, was weighed in a very sensible balance, before it was exposed to the action of the sulphuric acid under the receiver. Its temperature was 45°, and the height of the barometer 30·4. In half an hour's time, it was again weighed, and the loss by evaporation was found to be 1·24 grains. It was replaced, and the air was rarefied till the gauge of the pump stood at 15·2; in the same interval of time it was re-weighed, and the loss was 2·72, but its temperature was reduced to 43°. The loss from evaporation, in equal intervals, with a pressure constantly diminishing one-half, was found to be as follows:—

| Pressure | Temperature. Beginning | End | Loss Grains |
|---|---|---|---|
| 30·4 | 45 | 45 | 1·24 |
| 15·2 | 45 | 43 | 2·87 |
| 7·6 | 45 | 43 | 5·49 |
| 3·8 | 45 | 43 | 8·80 |
| 1·9 | 45 | 41 | 14·80 |
| ·95 | 44 | 37 | 24·16 |
| ·47 | 45 | 31 | 39·40 |

When the exhaustion was pushed to the utmost, the gauge stood at 0·07, and the evaporation in the half hour was 87·22 grains. During this last experiment, the water was frozen in about eight minutes, while the thermometer under the ice denoted a temperature of 37.

Now, before we infer from these experiments the state of evaporation, from different degrees of atmospheric pressure, it is necessary to apply to the results a correction for the variation of temperature which took place during their progress. The quantity of evaporation having been determined to be in exact proportion to the elasticity of the vapour, we must estimate the latter from the mean of the temperatures before and after the experiments, and calculate the amount for any fixed temperature accordingly. This will, doubtless, give us a near approximation, although, from the last experiment, we perceive that the method of estimating the temperature of the surface water cannot be absolutely correct. The following table presents us with the former results so corrected for the temperature of 45°:

| Pressure. | Grains. |
|---|---|
| 30·4 | 1·24 |
| 15·2 | 2·97 |
| 7·6 | 5·68 |
| 3·8 | 9·12 |
| 1·9 | 15·92 |
| ·95 | 29·33 |
| ·47 | 50·74 |
| ·07 | 112·32 |

Notwithstanding the slight irregularity of the above series, we can, I think, run no risk in drawing from it the conclusion, that the amount of evaporation is *cæteris paribus* in exact inverse proportion to the elasticity of the incumbent air; and that De Saussure was misled by his hygrometer, when he inferred from its indications, that a diminution of one-third the density doubled the rate.

Before we proceed, it is necessary to say a few words upon the apparent discrepancy between the results of Mr. Dalton's experiments and mine, as to the amount of evaporation, at the full pressure of the atmosphere. He found, upon the supposition of no previous vapour existing in the air, that the full evaporating force of water, of the temperature of 45°. would be 1·26 grains per minute, from a vessel of six inches in diameter. This amount reduced in proportion to the squares of the diameters of the two vessels, would give 7·65 grains in half an hour, from the glass of 2·7 inches diameter, which I employed. It must, however, be recollected, that Mr. Dalton's culculations were founded upon experiments made at a temperature very considerably above that of the surrounding medium, and that consequently a current must have been established in the latter which greatly accelerated the progress. It is true, that he afterwards subjected his calculations to the test of experience, at common atmospheric temperatures; but then he expressly states, that "when any experiment, designed as a test of the theory, was made, a quantity of water was put into one of them (vessels), the whole was weighed to a grain; then *it was placed in an open window, or other exposed situation,* for ten or fifteen minutes, and again weighed, to ascertain the loss by evaporation." In this way he ascertained, that with the same evaporating force, a strong wind would double the effect. The difference, however, even after these considerations, is still very striking; but, from several repetitions of the experiment, I have no doubt of its exactness.

### *Experiment* 4.

The arrangement described in the last experiment, having been found adequate to maintain in the receiver a state approaching to that of complete dryness, I had no opportunity of judging whether the elasticity of the vapour, as it rose from the surface of the water, varied in any degree with the pressure of the air, or whether any part of the increase of evaporation were dependant upon such variation. To determine this point, I placed the sulphuric acid in a glass, of the diameter of 2·8 inches, so that its surface was very little more than equal to that of the water. The vessels were placed, side by side, upon the plate of the air-pump, and covered with the receiver. The temperature of the water and air was 52°, and the height of the barometer 29·8. The following table shews the dew point, which was obtained, at intervals of half an hour, at different degrees of atmospheric pressure:—

| Barom. | Temp. of Water and Air | Dew Point |
|---|---|---|
| 29 8 | 52 | 36 |
| 14.9 | 53 | 37 |
| 7·45 | 52 | 35 |
| 3·72 | 53 | 36 |
| 1·86 | 52 | 34 |
| ·93 | 52 | 36 |
| ·15 | 52 | 36 |

The differences of these results are so extremely small, and are moreover so little connected with the variations of density, that there can be no difficulty in regarding them as errors of observation, and we may conclude, that the elasticity of vapour, given off by water of the same temperature, is not influenced by differences of atmospheric pressure. The equal surfaces of sulphuric acid and water here made use of, maintained, at the temperature of 52°, a degree of saturation equal to 570. I repeated the experiment, at the temperature of 61°, and the following are the results:—

| Barom. | Temp. of Water and Air | Dew Point |
|---|---|---|
| 29·6 | 61 | 48 |
| 14·8 | 61 | 49 |
| 7·4 | 60 | 48 |
| 3·7 | 61 | 50 |
| 1·85 | 61 | 48 |
| ·92 | 60 | 48 |
| ·15 | 61 | 48 |

Under these circumstances, the amount of saturation was 651; an increase evidently dependant upon the force of the vapour, but not in exact proportion to its augmentation.

### *Experiment* 5.

Being now desirous of ascertaining in what degree the temperature of an evaporating surface would be influenced by differences in the density of the air, I made the following disposition of the apparatus:—To a brass wire, sliding through a collar of leathers, in a ground brass plate, I attached a very delicate mercurial thermometer; this was fixed, air-tight, upon the top of a large glass receiver, which covered a surface of sulphuric acid of nearly equal dimensions with its base. Upon a tripod of glass, standing in the acid, was placed a vessel containing a little water, into which the thermometer could be dipped and withdrawn by means of the sliding wire. The bulb of the thermometer was covered with filtering-paper. At the commencement of the experiment, the barometer was at 30·2 inches, and the temperature of the air 50°. Upon withdrawing the thermometer from the water, it began to fall very rapidly, and in a few minutes reached its maximum of depression. The following table presents the results of the experiment, for different degrees of the air's density; the intervals were each of twenty minutes:—

| Barom. | Temp. of Air | Temp. of wet Ther. | Difference |
|---|---|---|---|
| 30·2 | 50 | 41 | 9 |
| 15·1 | 49 | 37 | 12 |
| 7·5 | 49 | 34 | 15 |
| 3·7 | 49·5 | 31·5 | 18 |
| 1·8 | 49·5 | 28·5 | 21 |
| ·9 | 49 | 24·5 | 24·5 |
| ·4 | 49 | 23 | 26 |

Here, in an atmosphere which a former experiment has proved to be in a state of almost perfect dryness, we find that, at the full atmospheric pressure, the wet surface of the thermometer was reduced 9°. It is worthy of remark, also, how small a quantity of water is required to produce this effect. It has been previously shewn, that a surface of 2·7 inches diameter, only lost 1·24 grains in half an hour. This would have been 1·41 grains at the temperature of 49°. The surface of the wet thermometer could not have exceeded $\frac{1}{50}$th of that of the evaporating vessel, and the maximum effect was produced in ten minutes, or $\frac{1}{3}$ of the time, so that the weight of water evaporated in this case was not more than (·0094 grains) one-hundreth of a grain. It will be seen, that the depression increased with the rarefaction of the air, but in the proportion only of the terms of an arithmetical progression to those of a geometrical. The increase is attributable, not to the augmented quantity of the evaporation, but to the decreased heating power of the atmosphere. MM. Du Long and Petit, in their experiments upon the cooling power of air, determined it to be nearly as the square root of the elasticity; but whether the heat which it is capable of communicating to a cold body, follow the same progression, the experiments above detailed are not sufficient to determine with precision. We may, however, certainly conclude from them, that the temperature of an evaporating surface is not affected by the mere quantity of evaporation.

It is right to remark that, in the last experiment, care was always taken to station the evaporating thermometer in the same place in the receiver, for I found that, when the air was highly rarefied, a greater degree of cold could be produced by approximating the wet bulb to the surface of the acid. No difference, however, could be perceived from such a change at the full atmospheric pressure. I also ascertained that no change of relative position in the surfaces of the acid and water produced any alteration in the dew point under any circumstances.

The few simple facts above determined appear to me to be intimately connected with the solution of some very important atmospheric phenomena, and I shall endeavour briefly to indicate their relation.

The aqueous fluid is so abundantly spread over the face of the earth, that there can be no doubt that the permanently-elastic atmosphere, which surrounds it, would very speedily be saturated with its steam, did not some cause, analogous to the sulphuric acid in the receiver, prevent its universal diffusion. This never-failing cause is inequality of temperature. As in the small experiment we found that the degree of dryness was proportioned to the energy of the absorbent mass, and that the existing vapour was equally diffused between it and the exhaling surface; so, in the larger operations of nature, we shall find that the state of saturation is dependant upon the point of precipitation, and that the aqueous atmosphere is nearly uniform between it and the source of steam.

Now, it is well understood that the temperature of the gaseous atmosphere in its natural state must decrease with its density as we ascend to its upper parts; so that a great degree of cold is at all times to be found within a very moderate distance from the surface of the waters. It is this low temperature which determines the tension of the aqueous atmosphere; and it is evident that the evaporation which is thus caused at the base of the aërial fluid, must be accompanied by a simultaneous and equal precipitation above. What then becomes of the precipitated moisture? Let us endeavour to trace the order of this phenomena. We will first suppose a calm state of the atmosphere, a temperature of 80°, and the barometer at 30 at the surface of the earth. By a calm state of the atmosphere is here meant, one that is free from any lateral wind, and in which, the only currents being in an ascending and descending direction, evaporation would proceed at the rate exhibited in the first column of Mr. Dalton's table. The dew-point at the surface of the earth is 64°, and this is determined by the temperature at the height of about 5000 feet, where the barometric column would maintain itself at 24 inches. The degree of saturation below would therefore be 600, and the amount of evaporation 1·74 grains per minute from a surface of six inches diameter. This quantity we therefore suppose condensed at the height before named. But the state of saturation in the atmosphere, above this point of precipitation, is again diminished; for we may suppose the force of the vapour to be determined by a temperature of 31° at a height of 15,000 feet, where the barometer would stand about 16 inches. The force of evaporation would, therefore, be 1.71 grains per minute, at the full atmospheric pressure; and this amount increasing as the pressure diminishes, would give 2·13 grains per minute; so that the power of evaporation at this stage exceeds the supply of moisture, and no cloud could possibly be formed. Above the second point of condensation let us now suppose the force of the vapour to be determined, in still loftier regions, by a temperature of 12°. The force of evaporation would then be 0·44 grains, increased in the proportion of 16 inches to 30, or 0·82 grains. Here, then, the power of evaporation would be insufficient to diffuse in the upper regions the whole of the moisture supplied from the surface of the earth, and a cloud, it might be supposed, must consequently result. But another modification of the process now ensues; the precipitated moisture has a tendency to fall back into the warm air below it, and consequently would again assume the elastic form with a rapidity proportioned to the rarefaction of the stratum in which it is diffused. There is, I think, no difficulty in supposing that no visible cloud, or one of extreme tenuity, would be formed during this double process of evaporation. A very important re-action, however, must take place upon the strata of vapour beneath; the elastic force being increased above, enables the water below to maintain an atmosphere of a higher degree, and the quantity of evaporation must decrease as the point of saturation rises. A different arrangement of the points of precipitation would ensue in the progress of these effects.

An important distinction must here be drawn between the ultimate effects of the superior and inferior evaporation denoted above. In the first, the whole weight of water is condensed and simultaneously exhaled; and although it constitutes steam of an inferior degree of force, there is little or no difference in the quantity of its latent heat, and no effect is therefore produced upon the temperature of that portion of the atmosphere in which the change takes place. But in the second, the condensation happens at one spot, and the vaporization at another inferior to it; the latent heat is therefore evolved at the former and communicated to the air, while at the latter the process is reversed, and the air is cooled. The process of this operation would, therefore, tend to equalize the temperature of the atmosphere.

We will next imagine that the surface of the earth is swept by a high wind, and that the atmosphere instead of resting calmly upon its base, moves laterally with great velocity. Under these circumstances experience has shewn that the amount of evaporation will be nearly doubled; but the force of evaporation is not altered in the upper regions. The inferior exhaling surface being immoveable, the motion of the air perpetually changes, and renews the points of contact, and prevents accumulation at any one place; but in the heights of the atmosphere the exhaling surface of the cloud is borne upon the wind, and their relative situations never change.

The progress of precipitation must, therefore, necessarily, under these circumstances, outstrip that of evaporation, and the disturbance of the atmospheric temperature will be greatly accelerated.

There is another cause which would also quicken evaporation below, without equally increasing its power of diffusion at any given height above; and that is a decrease in the density of the air at the surface of the earth. Under the circumstances of our first supposition imagine the barometer to fall to 28 inches, the evaporation would be increased from 1·74 grains per minute, to 1·86 grains; but this decline of two inches at the surface would indicate a contemporaneous fall of little more than one inch at the height of 15,000 feet, and the rate of diffusion would vary accordingly. When it is considered that great falls of the barometer are generally accompanied by high winds, and that this disparity is multiplied by the force of the current, it is easy to appreciate the influence of this local increase of the power of evaporation.

The facility of evaporation in the rarer regions of the atmosphere will also go far to account for the state of saturation in which the air of mountainous countries is generally found, and many minor meteorological phenomena might probably meet with their explanation from variations of the same cause; such as the fogs which frequently accompany a very high degree of atmospheric pressure, and that peculiar transparency of the air which often precedes rain, and is accompanied by a falling barometer. But to return again to the more general and extended influence of the vapour upon the boundless strata of the atmosphere:—that the phenomena of evaporation and condensation, as we have been contemplating their progress, have not been described with any bias to theoretical considerations, but are in strict accordance with facts and observations, any one might easily convince himself with less difficulty than would at first be supposed. To prove the assertion I shall extract the following passages from the works of De Luc, who was probably one of the most accurate observers of nature that ever existed, and who seldom, indeed, allowed any hypothetical considerations to warp his description of what he had observed. They will afford a complete illustration of the preceding remarks, although they were penned by him to support a very different hypothesis.

"Si l'on ne fait qu'une légère attention à la surface de ces brouillards vus des montagnes pour en jouir comme d'un beau spectacle, on peut penser qu'ils sont permanens; que l'évaporation est arrivée à son maximum à la surface des eaux, parce que l'air est parvenu à l'humidité extrême; et que les vapeurs vesiculaires qui troublent la transparence de cet air restent les mêmes durant des semaines ou même des mois; cest-a dire, tant que le brouillard se conserve à une même hauteur. Mais le phénomène diffère beaucoup de cette première apparence: l'evaporation continue à la surface des eaux, les vapeurs vésiculaires qui s'en forment montent sans cesse et une nouvelle évaporation a lieu à la surface des brouillards. C'est un spectacle aussi amusant qu'instructif, que celui que fournit cette surface, vue d'un lieu peu élevé audessus d'elle, et dans une grande vallée ou l'on ait à quelque distance, des montagnes rembrunies par des forêts de sapins. Une telle vallée éclairée par les rayons du soleil semble être comblée de coton, filé dans toute sa surface par des êtres invisibles en fils invisibles: il s'y fait par-tout des tumeurs, semblables a celle que produit une fileuse sur sa quenouille en tirant le coton pour former son fil, et elles disparoissent successivement en se dissipant dans l'air. Quelquefois ces tumeurs s'allongent et se separent de la masse en tendant à monter: on les voit alors

s'étendre comme un paquet de gaze qui se déploie et peu à peu elles disparoissent. Les brouillards se forment donc constamment à la surface des eaux et du sol; mais constamment aussi ils se dissipent dans l'air supérieur: et cependant on n'apperçoit point que l'humidité y augmente."—*Idées sur la Météorologie*, Tom. 11, p. 78.

"Depuis que mes idées ont changé sur la cause de la pluie, j'ai fort souvent fixé mon attention sur les nuages et j'ai reconnu trés évidemment, qu'ils s'évaporent même tandis qu'ils grossissent. Si l'on fixe ses regards sur leur bord découpé qui, lorsqu'il a pour fond l'azur du ciel, presente mille figures singulières, celles que l'imagination leur prête alors, peut aider à l'examen dont je parle, en rendant leurs changemens plus frappans. Il arrive souvent, que la partie sur laquelle on fixe son attention se dissipe au lieu même où l'on a commencé à l'observer: souvent aussi on la voit s'étendre, sans que la totalité du nuage se meuve, et elle ne se dissipe pas moins durant cette extension. Quelquefois, tandis que l'un des festons du nuage se dissipe on en voit d'autres se former, s'étendre, produire eux-mêmes de nouveaux festons; par ou le nuage grossit: d'autres fois il diminue; et alors tous ses festons s'évaporent successivement et il n'en acquiert de nouveaux, que parcequ'il se découpe: on apperçoit en même tems, qu'il devient plus mince et il disparoit enfin totalement.

"C'est ce qui m'a conduit à penser qu'il y a en effet dans l'air, une source générale de vapeurs qui en fournit en certaines circonstances; que ces vapeurs sont produites au lieu même où se forme un nauge; que c'est par la durée de cette production de vapeurs, que les nuages subsistent, s'aggrandissent même, quoiqu'en s'évaporant tout le tour; et que lorsqu'ils se dissipent c'est que leur evaporation n'est plus réparée par la formation de nouvelles vapeurs."—*Ib.* p. 117.

I shall now conclude this paper with an observation which is intimately connected with the subject of the preceding pages. It has been argued that the quantity of heat which would be communicated to the air by the condensation of atmospheric vapour would be trifling, and inadequate to produce those expansions in

the aërial currents to which, in my essay upon the constitution of the atmosphere, I have ascribed the fluctuations of the barometer. Now, I have therein shewn how the gradual spread of a small increase of temperature, through a considerable stratum, is sufficient for the purpose; and a very little consideration will, I think, convince any one that the evolution of caloric is by no means so small as has been supposed.

The following rough calculation will place the facts in a striking point of view:—The latent heat of steam has been proved to be somewhere about 970°, and it is known that, whatever be its density, or the temperature at which it is produced, the amount will differ but little from this estimate. The condensation, therefore, of a pound of steam of any degree of elasticity would be adequate to raise a pound of water 970°. The capacity of atmospheric air, of mean density, for heat, compared to that of water, is as ·2669 to 1; therefore the same quantity of heat which would raise a pound of water 1°, would raise a pound of air 3°·7. The condensation of a pound of steam would, therefore, elevate the same weight of air to 3589°. A pound of air is equal to about 11 cubic feet, so that the evolution of heat from the condensation of a pound of steam, would be sufficient to raise the temperature of 3657 cubic feet of air 10°.

When we now look to the depth of water which falls upon the surface of the earth, and recollect that this is not the sole measure of the effect we are endeavouring to estimate, but that the unceasing precipitation and exhalation of the clouds is perpetually extending this influence to the most inaccessible heights, we shall, perhaps, have a juster notion of the prodigious power of atmospheric vapour; and it will, I think, be granted that I have not over-rated the impulse which it is calculated to impart.

## Art. VII.—*A Design for making a Public Road under the Thames, from the east side of the Tower, near Iron-Gate Stairs, to the opposite side of the River, near Horseleydown Stairs.* By Samuel Ware, Esq.

[Communicated by the Author.]

| | | |
|---|---|---|
| *Internal Dimensions of the Arch-way.* | The carriage-road . . . | 28 feet wide. |
| | The height above the road . | 18 feet. |
| | The foot-paths . . . . | 14 feet wide. |
| | The greatest width . . . | 42 feet. |
| | The greatest height . . . | 21 feet. |

The following particulars of the Estimate describe the mode of erecting the arch-way:—

Compensation for the ground and buildings on the north side of the river, and for the ground and buildings on the south side, to form the approaches; cofferdams, in ten successive lengths or removes, to keep out the water; and strutting, to keep up the ground.

Steam-Engines, to keep the works within the cofferdams dry, and subsequently for draining the road, should there be occasion.

Digging out a channel, in the bed of the river, for the arch-way, and the ground for the approaches.

Removing the refuse earth; claying, filling in, and leveling, two feet above the extrados of the arch.

Yorkshire Ledgers for the foundations of the arch-way, and walls of the approaches and embankments, and piling as occasion may require.

Stone-work, cut in voussoirs, of the arch, and counter-arch.

Lining with lead, 10*lb.* to the foot superficial, enveloping these arches.

Super-arch of brick-work, lined externally with tiles in cement.

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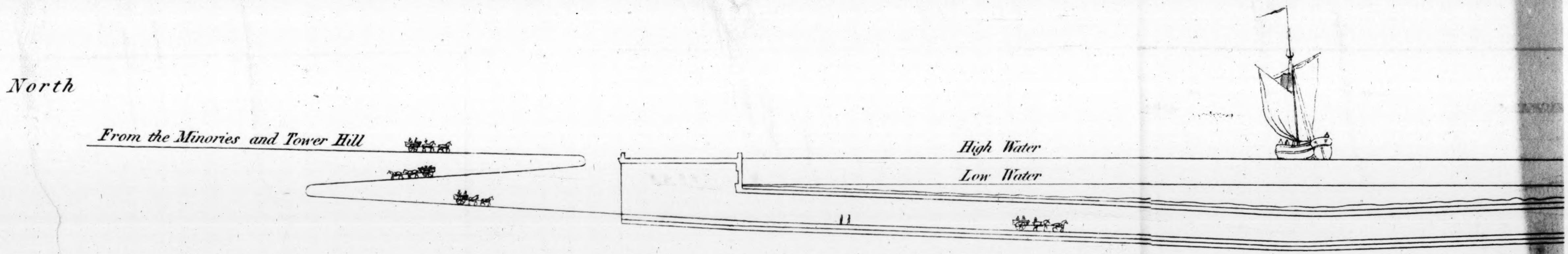
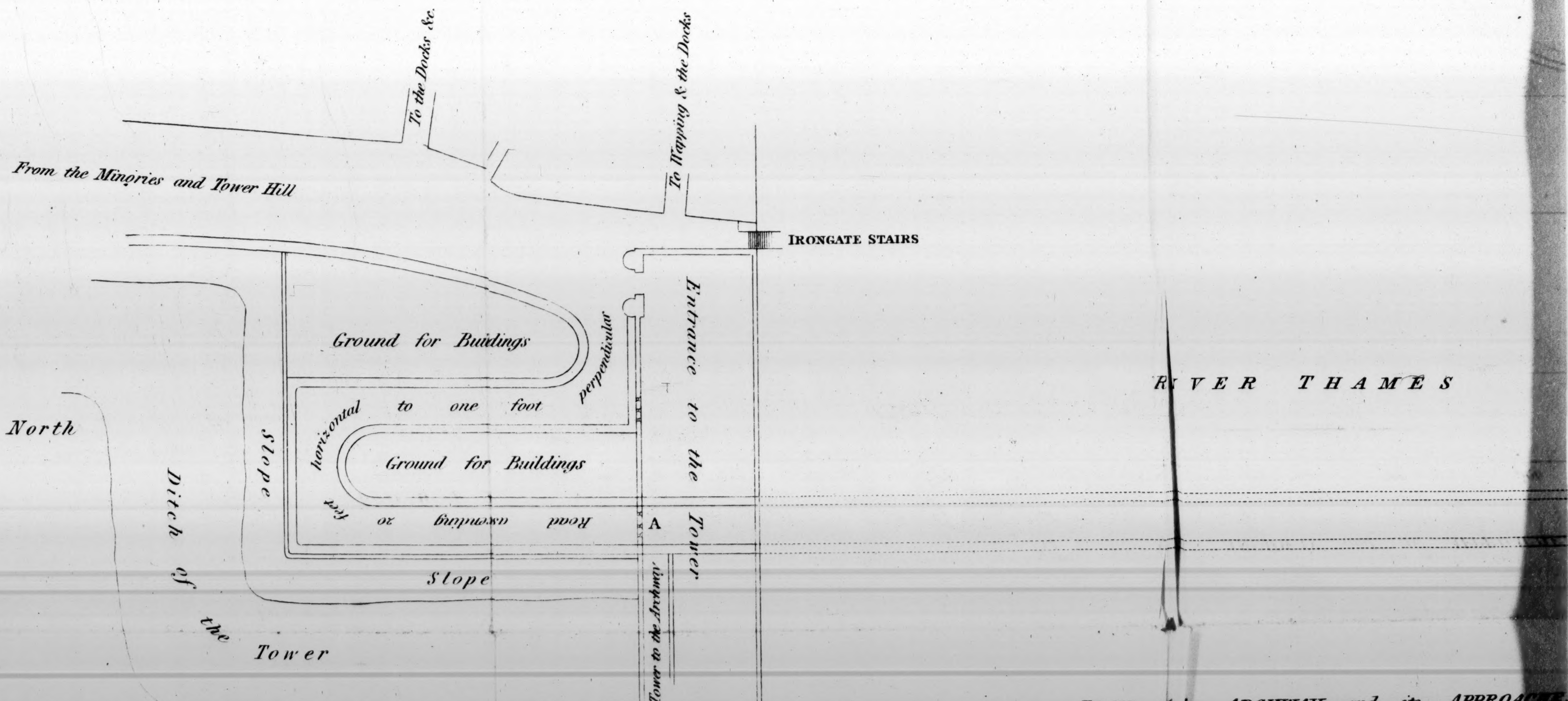
*b* £400 per day, or £146,000 per annum.

# DESIGN *for making a* ROADWAY *under*
*from the east side of the Tower near Irongate Stairs, to the opposite*



*LONGITUDINA SECTION of the ARCHWAY*

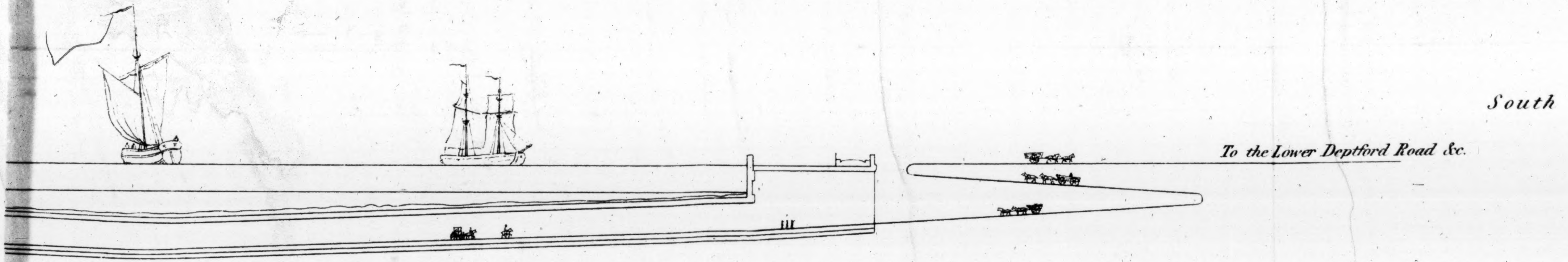


*PLAN of the ARCHWAY and its APPROACHE*

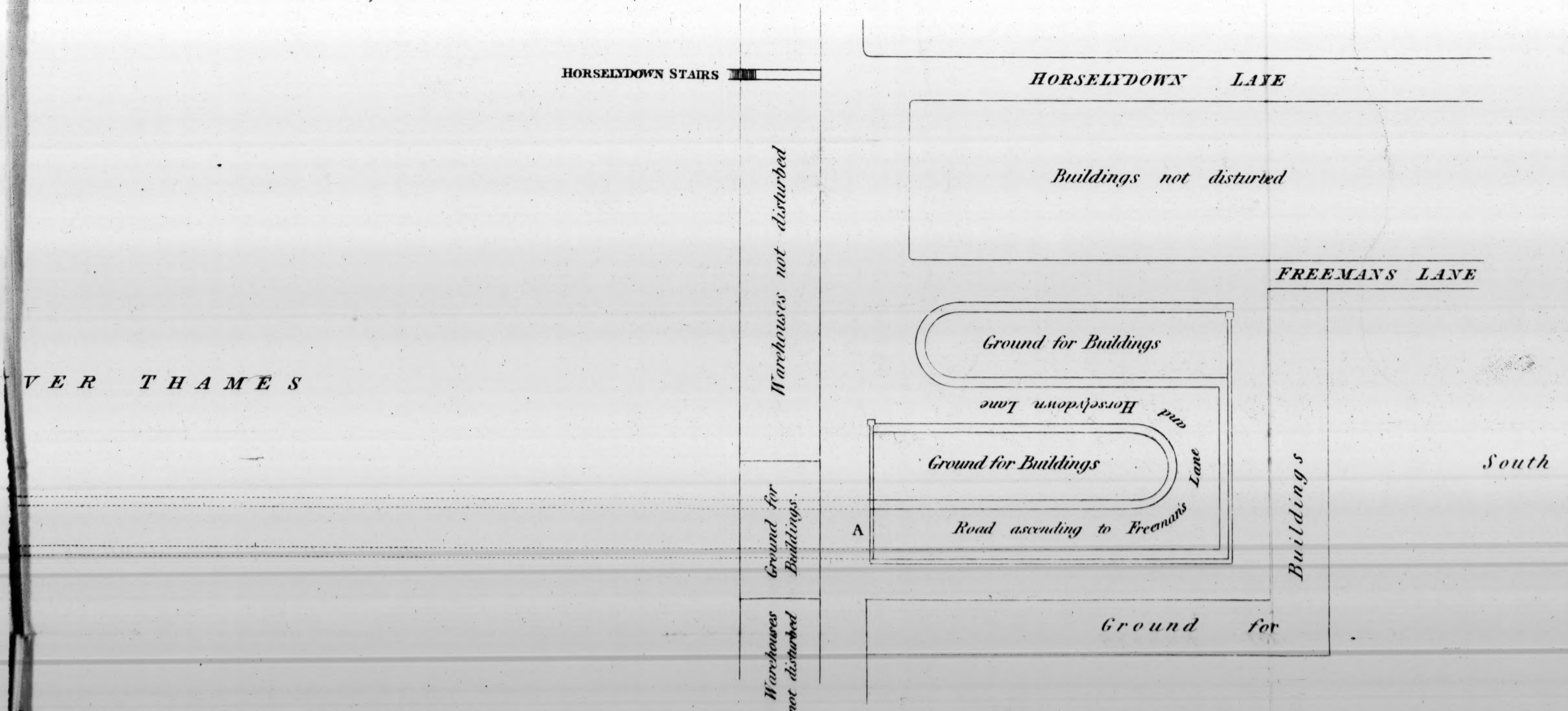
*Scale.*

# a ROADWAY under the THAMES,

Irongate Stairs, to the opposite side near Horslydown Stairs.

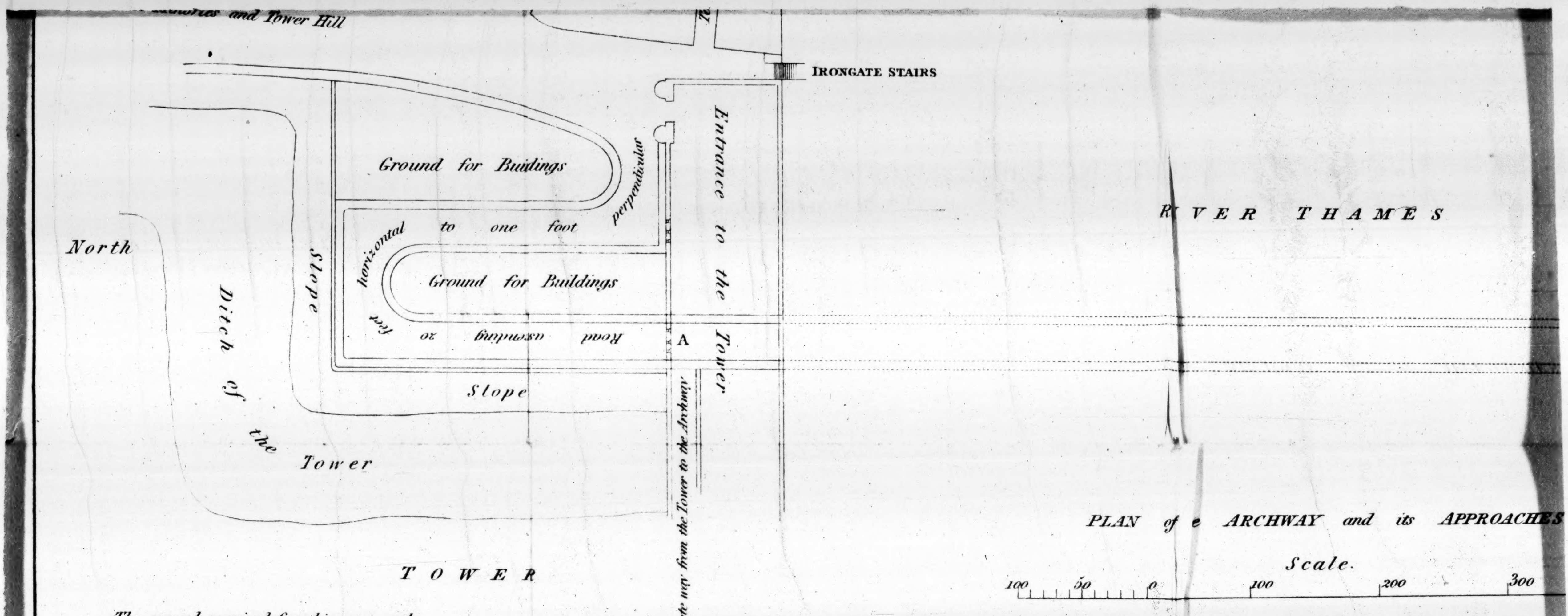


SECTION of the ARCHWAY.



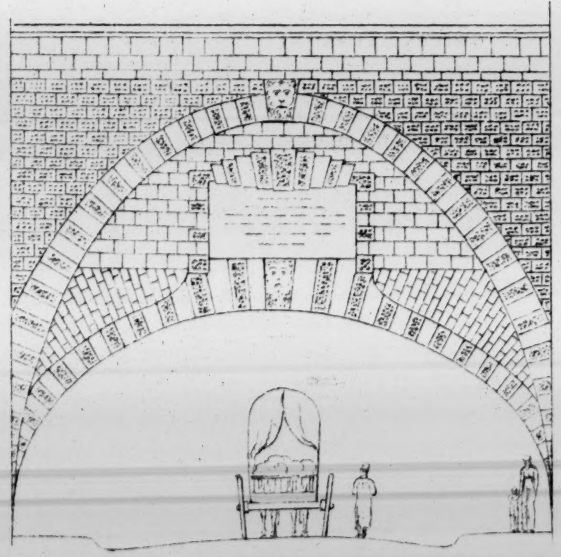
ARCHWAY and its APPROACHES.

Scale.

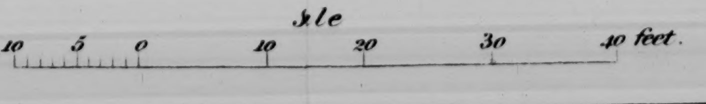


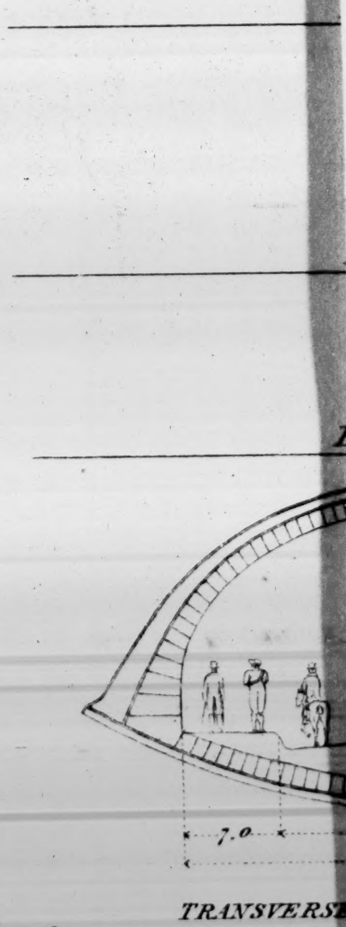
PLAN of e ARCHWAY and its APPROACHES

*The ground required for this approach belongs wholly to the Tower.*

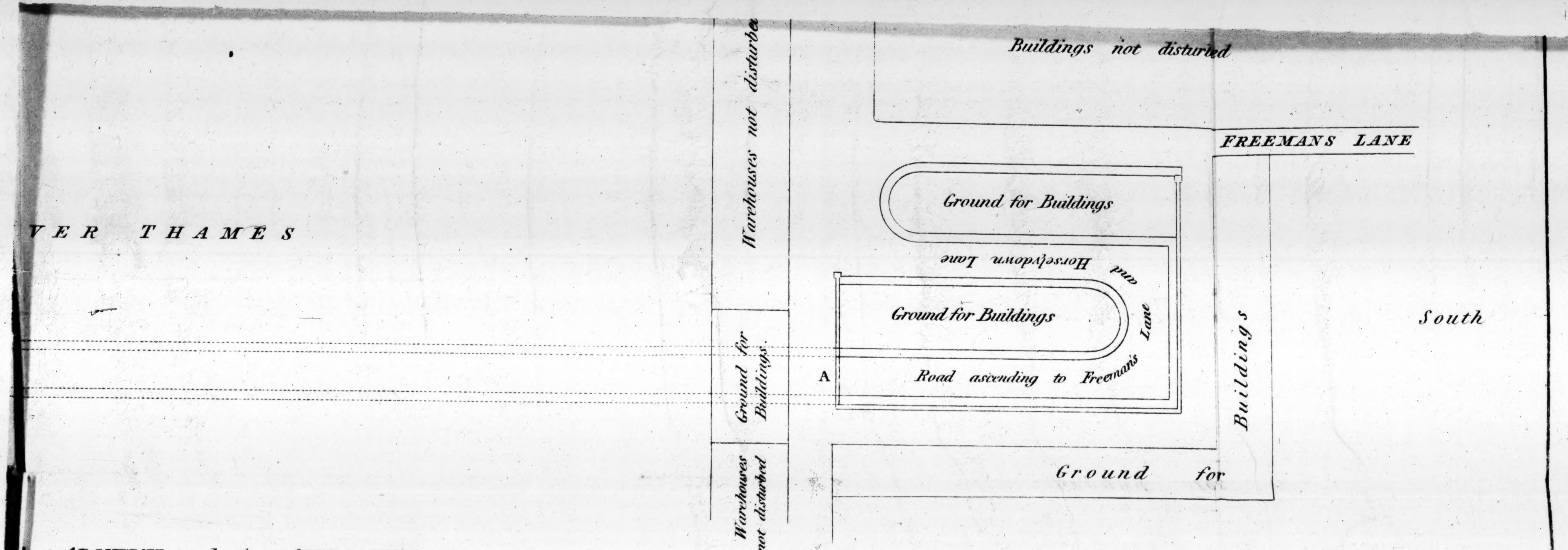


ELEVATION of the ENTRANCE at A.





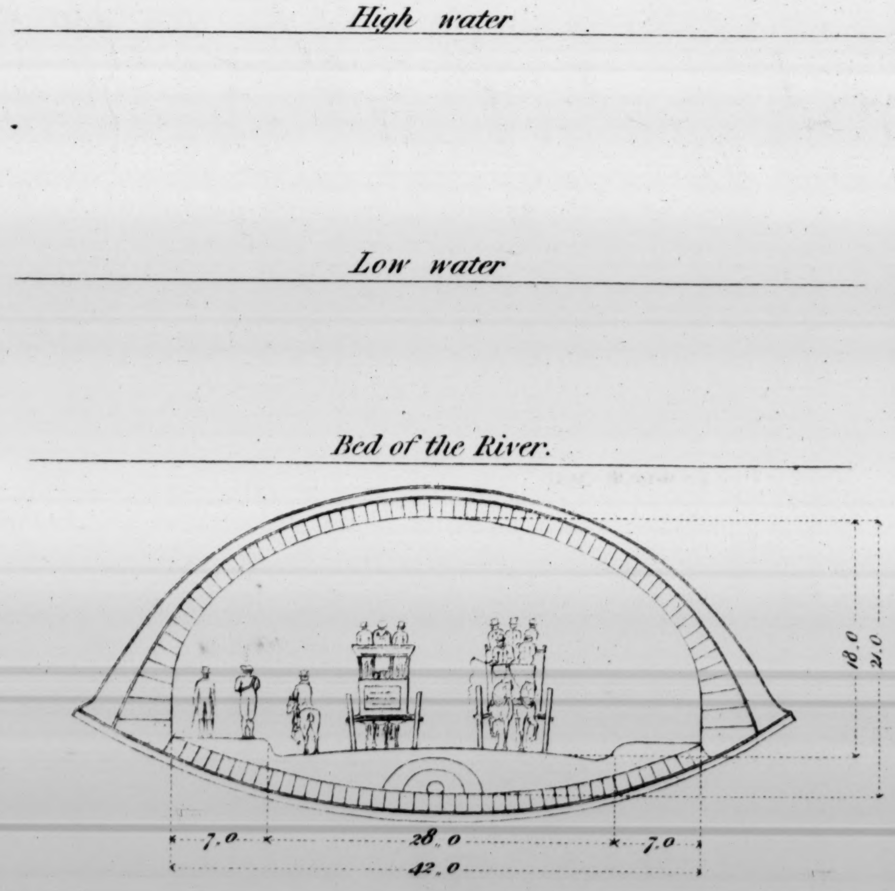
TRANSVERSE



e ARCHWAY and its APPROACHES.

Scale.
100 200 300 400 feet.

N.B. In the day time the Archway will be sufficiently lighted from the ends.



TRANSVERSE SECTION of the ARCHWAY.

le
20 30 40 feet.

Jª Basire. sculp.

cement.

Centering for the arches.

Forming and gravelling the road, ascending one foot perpendicular to twenty feet horizontal.

Drains, pipes, foot-paths, and lamps.

Embankments, and other walls and parapets, in the approaches *.

Facings to the entrances to the arch-ways, and toll-houses.

| | |
|---|---|
| Estimated amount of the above works . . . . . | £250,000 |

## *Pecuniary Advantages.*

A small part of such a revenue †, as would be derived from the number of passengers and carriages which has been estimated to pass London Bridge daily, calculated at the tolls allowed to the Southwark Bridge Company, would be ample to compensate the cost of this arch-way.

The taxes arising out of the materials of the buildings likely to be erected, together with the assessed taxes arising out of them when built, in the ways to and through the lower road to Deptford, and in communicating with the Kent Road, consequent on such a connexion between the two sides of the Thames, would be a great source of revenue to the government, probably more than sufficient to compensate the cost of this arch-way.

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* This mode of approach is also applicable to a road descending from a bridge; is cheap, by lessening the expense of compensation for the buildings and ground required and damaged in making an inclined plane; and is convenient, because the foot of the inclined plane is at the river.

| | | BRIDGES. | | | Average | Toll | £. | s. |
|---|---|---|---|---|---|---|---|---|
| | | London | Blackfriars | Westminst. | | d. | | |
| † Foot Passengers - - | Passing Daily [a] | 89640 | 61069 | 37820 | 62843 | 1 | 261 | 17 |
| Waggons - - - - - - - | | 769 | 533 | 173 | 492 | 12 | 24 | 12 |
| Carts and Drays - - - | | 2924 | 1502 | 963 | 1796 | 8 | 59 | 17 |
| Coaches - - - - - - - | | 1240 | 990 | 1171 | 1133 | 9 | 42 | 9 |
| Gigs and Taxed Carts | | 485 | 500 | 569 | 518 | 4 | 8 | 12 |
| Horses not drawing - | | 764 | 522 | 615 | 633 | 1½ | 3 | 19 |
| | | | | | | | £401 | 6 [b] |

[a] See Month. Mag. March, 1816, and Morn. Chron. May 26, 1812.

[b] £400 per day, or £146,000 per annum.

The saving in time, and in the wear and tear of carriages, horses, and men who would otherwise go over London Bridge, or cross the river in boats, would be a compensation for the tolls to be paid at this arch-way.

The carriages and passengers are those coming from the streets adjacent to the site of the proposed arch-way, those going into or through Surrey from the East India, West India, and London Docks, from the Commercial Road, and from the Counties of Hertford, Cambridge, Norfolk, Suffolk, and Essex; also those going to the above-mentioned places from the counties of Surrey and Kent.

The necessity of increasing the width of the intended new London Bridge, by this diminution of the number of passengers and carriages, may be obviated, and a large sum of money thereby saved.

### *Political Advantages.*

The communications, by this road, between the officers of government, and the Mint, Trinity-House, Custom-House, and the Tower, may be facilitated.

A readier transfer of soldiers, arms, and stores, to and from the counties north and east of London, and the Tower, to and from Woolwich, Chatham, and Sheerness, by land, will be obtained by this arch-way.

This arch-way may be made a military pass, there being proposed a private way to it from the Tower.

### *Observations.*

It seems remarkable, considering the great advantages to be obtained in populous cities by opening a communication between the shores of a navigable river, for foot-passengers, horses and carriages, without interrupting the navigation on the river, that the passage under the Euphrates, constructed by Semiramis, at Babylon, is the only one upon record. The account Diodorus the Sicilian gives of it may be translated thus:—

"In the low ground of Babylon, Semiramis sunk a square pond, 35 feet deep, each side being 300 stadia in length, the banks whereof were lined with bricks well cemented with bitumen[*], she then turned into it the water of the Euphrates[†]. Across the channel of the river, thus made dry, she then made a passage in the nature of a vault from one palace to the other. The arch was built four cubits[‡] thick, of firm and strong bricks, plastered all over on both sides with bitumen. The walls supporting the arch were 20 brick[§] in thickness, and 12 feet high from the floor to the springing of the arch, and the breadth of the passage was 15 feet.

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[*] Dr. Hulme, (Archæologia, vol. xiv. page 57,) analyzed the cement adhering to a brick brought from Babylon, and found it to be bitumen.

[†] Strabo, (lib. xvi. page 738,) states the width of the Euphrates to be one stadium, which is generally taken at a furlong, or 660 feet. M. Gosselin shows that there were two stadia; one used by Herodotus, called the short stadium, about 329 feet English: the other of Archimedes, about 438 feet English. Ctesias, from whom Diodorus had his account, used the stadium of Archimedes. Herodotus used the short stadium. In this way the discordance of Herodotus and Ctesias, in respect to the wall of Babylon, has been reconciled.

[‡] A cubit royal of Babylon was estimated, by Romé de Lille, at 22$\frac{2}{10}$ inches English.

[§] There is a brick in the British Museum, brought from the site of ancient Babylon. That described by Dr. Hulme, in Archæologia, vol. xiv. page 55, is 13½ inches square, and 3 inches in thickness, and weighs 38 lb. 11 oz. avoirdupois. He analyzed the material, and found it to be pure clay, and not burnt. Dr. Henley, in the same volume, page 205, deciphered the inscription on it, "a brick baked by the sun." Pocock measured some of the bricks of the brick Pyramid at Saccara, built by king Asychis; he found some 13½ inches long, 6½ broad, and 4 thick; and others 15 inches long, 7 broad, and 4¾ thick.

In Rennel's Geo. Sys. of Hero, section 14, page 356, is the following note: "Diodorus describes a vaulted passage under the bed of the Euphrates, by which the Queen Semiramis could pass from one palace to the other, on different sides of the river, which was a stadium in breadth, (according to Strabo, page 738,) without crossing it. This serves, at least, to show, that the palaces were very near the river's banks."

"At a time (1800) when a tunnel, of more than half a mile in length, under the Thames (at Gravesend) is projected, it may not be amiss to mention the reported dimensions of the tunnel made by Semiramis, under the Euphrates; which, however, was no more than 500 feet in length, or less than 1-5th of the projected tunnel under the Thames. That of Semiramis was said to have been 15 feet in breadth, 12 feet in height to the springing of the arch, perhaps 20 in all. The ends of the vault were shut up with brazen gates. Diodorus had an

This work was finished in 260 days, and then the river was turned into its ancient channel; so that Semiramis could go privately from one palace to another, under the river. She made also two brazen gates at each end of the vault, which continued to the time of the kings of Persia, the successors of Cyrus."

In 1798, a tunnel, 900 yards in length, was projected to pass under the Thames, to unite Tilbury, in Essex, with Gravesend, in Kent, at an estimate of only £15,955. Subscribers were obtained to promote the undertaking, by whose means an engine-house and steam-engine were erected, and a shaft dug, about 146 feet deep, when the engine-house was burnt, and the operations were abandoned.

In 1805, an Act of Parliament (45 Geo. 3. cap. cxvii.) was obtained, to construct a tunnel under the Thames, at the Old Horse Ferry, about 2¾ miles below London Bridge, and to raise £140,000, and a further sum of £60,000, in all £200,000. A shaft, 76 feet deep, was sunk, and a driftway, 5 feet high, 3 feet wide at the bottom, and 2 feet 6 inches at the top, was extended, under the direction of Mr. Trevetheck, a Cornish miner, 1011 feet from the south shore, under the bed of the Thames, when sand and water burst in upon the workmen, and further progress was suspended. The powers given by this act are now, by lapse of time, void. In 1809, notice was given,

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idea* that the Euphrates was 5 stadia in breadth, see lib. ii. c. 1. The Euphrates was turned out of its channel, in order to effect this purpose. Herodotus, who is silent concerning the tunnel, says, that the river was turned aside in order to build a bridge. Diodorus describes a bridge also. There is an absurd story told, in both these historians, respecting the disposal of the water of the river during the time of building the bridge. According to them, the water was received into a vast reservoir, instead of the obvious and usual mode of making a new channel to conduct the river, clear of the work constructing in its bed, into the old channel, at a point lower down †."

* Diodorus merely states, that the bridge built by Semiramis was 5 stadia in length. Bridges are frequently five times as long as the width of the river they stride.

† This story, from the vastness of the reservoir, may be true. Local circumstances may have compelled Semiramis to adopt this apparently extravagant mode of removing the water of the Euphrates from the site of the tunnel.

by public advertisement, that the directors were desirous of receiving designs for proceeding again in this work, and they offered a premium of £200 for the plan which should be adopted, and a further premium of £300 upon the execution of it. Since that time the project has lain dormant. Lately a pamphlet has appeared, entitled "A New Plan of Tunnelling, calculated for opening a Road-way under the Thames, by M. T. Brunel, Esq., in order to the raising a capital of £——— by transferable shares of £100 each," for commencing again this project. Mr. Brunel describes his plan as follows: "The character of the plan before us consists in the mode of effecting this excavation by removing no more earth than is to be replaced by the body of the tunnel, retaining thereby the surrounding ground in its natural state of density and solidity."

Mr. Brunel proposed that the excavation, 34 feet wide by 18 feet high (external dimensions), consisting of 33 such drift-ways as that before mentioned, moving simultaneously, worked by 33 men, at the rate forward of three feet per day, followed by a brick tunnel at the same pace, should pass in a stratum which he states, "has been found to resist infiltrations," so that the crown of the tunnel will have a head of earth on it, from 12 to 17 feet in thickness, quite undisturbed, as he expects.

The method proposed by Mr. Dodd, at Gravesend, by Mr. Vazie, at Rotherhithe, and by Mr. Brunel, is by mining. Other methods may or have been proposed, such as to dredge out a channel in the bed of the river by machinery in vessels, and afterwards to sink therein caissons with brick or stone tunnels in them, to be afterwards secured together and perforated; or to sink large iron cylinders or boxes, the size of the proposed tunnel, with moving, lapping, and closing joints, let down, one after another, on strong iron mooring chains, into the channel so dredged out; the junctions to be facilitated by means of the diving-bell: but these schemes are of a very adventurous character, and might be tried perhaps with propriety in the case of a small passage under a river. The apparent cheapness of such methods seems calculated to obtain subscribers to such a project, but not to effect a dry and secure passage for men and carriages under the Thames.

The method of Semiramis was simple in design and certain of success. Troughs holding water, such as the canal aqueducts over rivers, are to be seen in all parts of the country; and there can be no doubt, that such an arch-way as that before described * in the estimate commencing this statement, executed in the open air, and uninterrupted by water during such erection, by means of cofferdams, would have a successful issue, and be perfectly dry under a river, for a thoroughfare for passengers and carriages. In modern times, a cheaper way of rendering the bed of a river dry has been discovered than that of Semiramis, which was by means of a reservoir to receive the waters of it, or even than that of Trajan, in building the bridge across the Danube, which was by making a temporary new channel to receive its stream. We have lately seen the piers of Waterloo Bridge and of Southwark Bridge laid dry, in the bed of the Thames, by means of cofferdams, the use of which, in keeping the space enclosed in them free from water, has been greatly extended by the facility obtained by means of steam-engines; and by similar means may an arch-way be constructed of almost any useful dimensions under the river, and with the same success, and not with more interruption to the navigation of the Thames than would be caused by one of the vessels in the pool getting athwart the stream, and remaining so for a few months.

Since the foregoing statement was made, an advertisement has appeared of the intention of applying to Parliament for leave to bring in a bill to erect a patent wrought-iron bar bridge of suspension, from some part of the parish of St. Botolph, Aldgate, over the Thames, to some part of St. Mary, Bermondsey, of such

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* The following account of the suspended gardens of Nebuchadnezzar, at Babylon, extracted from Diodorus, will show the care used by him, to render the rooms under them dry. " On the walls were laid stones, 16 feet long and 4 feet broad ; these were covered with reeds coated with brimstone, on which were laid double tiles, cemented together, and on them were laid sheets of lead."

a height as to admit vessels to pass under it at spring tides, without lowering their masts.

These repeated attempts to obtain a road-way for passengers and carriages eastward of London Bridge, across the river, together with the almost impassable state of London Bridge, from the crowds on it, in the middle of the day, show that there is a demand for such a communication between the sides of the Thames, eastward of London Bridge. The questions to be considered, are, First, what method of obtaining such an object is the best? Secondly, how that method can be carried into effect with certainty? Thirdly, what is the best site for such a road-way? Fourthly, whether such a work, in the site hereby laid down, would not be of such political importance, facilitated as the execution thereof would be by a possession of the ground necessary for the northern approach, as to warrant the State in undertaking the work, leaving to the public the use, subject to certain tolls and restrictions, as may accord with the uses of it by Government? Fifthly, as to time, should the cofferdams necessary to resist the deep water in the erection of the new London Bridge be of such a size as to cause an impetus to the river, or alteration of the mid-stream, so as to destroy the present bridge, or render it impassable, (it being intended that it shall remain until the new one is passable), would not then such a way as the one hereby proposed be a great relief to Southwark Bridge, until a temporary bridge be supplied? And, comparing generally the expediency and cost of carrying into effect this design with the expediency and cost of rebuilding London Bridge *, ought not this work to have the precedence?

SAMUEL WARE.

*5, John Street, Adelphi.*

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* See this Journal of Science, Roy. Inst., Nos. 29, 30, and 31, 1823; and Tracts on Vaults and Bridges, 1822.

## Art. VIII. *An Account of the Overflowing Well in the Garden of the Horticultural Society at Chiswick.* (*Communicated by* Joseph Sabine, Esq., S. H. S. *&c.*)

[The specimens adverted to in the following paper are deposited in the Mineral Room, at the Royal Institution.]

In consequence of the success which had attended the operations of several persons in the vicinity of Chiswick in boring for water, it was determined by the Council of the Horticultural Society that an attempt to procure an overflowing well should be made in the society's garden, for the purpose of obtaining a supply of water for various purposes; but more particularly to form an ornamental canal in the Arboretum for the growth of hardy aquatic plants.

After the necessary inquiries had been made, it was determined that Mr. John Worsencroft, a person who had previously succeeded in making an overflowing well for Messrs. Bird, of Hammersmith, should be employed to execute the experiment. He commenced his operations upon the first of September last; and after boring for five weeks without material interruption, tapped the spring on the 18th of October, and finally completed his task on the following day. The depth from which the water first rose was 317 feet, and the whole depth of the well, when completed, was 329 feet; the additional 12 feet of boring having been made in order to gain a perfect opening into the bed of the spring, which flowed when first tapped less copiously than after the final depth was obtained. The chalk from which the water immediately comes is soft, but the bottom of the well is in hard chalk. The water in all the neighbouring wells appears to have been obtained at about the same depth; and the strata through which the perforations were made are nearly similar to those met with in the present instance.

The tackle and instruments used were very simple. A scaffolding was raised 20 feet above the proposed orifice of the well, on which a platform was fixed to support a windlass, by which the rods used in boring were lowered into, and raised from, the well. These rods were of tough iron, about an inch and a half square, and ten feet long; the ends of each screwing on to, or unscrewing from, the top of the next, as they were lowered into, or raised from, the hole. The instruments fixed as occasion required to the lowest extremity of the series of rods when in action, were augers of various dimensions for boring, steel chisels for punching, and a hollow iron cylinder, (called a shell,) fitted with a valve at its lower end, for bringing up soft mud. The rods, when an auger was attached to them, were turned round by means of moveable arms or dogs, which were made to lay hold of the part of the uppermost rod at the top of the hole; the auger being thus forced through the stratum of clay or sand, was drawn up as soon as its cavity was filled with the substance it had loosened. The chisels were employed for punching through stones, hard substances, or hard chalk; the rods, when these were attached, were moved by means of a powerful beam acting as a lever, and worked by four men.

The water is discharged at the surface of the ground after the rate of six gallons per minute, and is capable of being carried 20 feet above the ground level; and even then supplies a copious stream. The well is lined for the first 186 feet with cast-iron pipes, with a three-inch bore, jointed by means of wrought-iron collars, which are rivetted into the pipes; the succeeding 77 feet 6 inches are lined with copper pipes, with 2½ inches bore, soldered into a single length, and resting in the chalk, through which the remainder of the hole is bored, and in which no pipes were used. The whole series of pipes was introduced at once, the hole having been prepared for receiving them as soon as it was ascertained that the augers had reached the chalk stratum. The land springs in the gravel, above the blue clay, were kept out in the first instance by extra iron pipes. The spring which was found in the sand below the blue clay, and above the chalk, rose to within a few feet of the surface, but did not overflow. The whole of the water of this spring is, however, excluded from the well by the pipes with which it is lined.

The cost of the well, including that of the pipes, boring, and every other expense whatever, did not exceed 130*l.*; and the manner in which it was executed, was, in every respect satisfactory. Indeed it is impossible to speak too highly of the care, attention, and dexterity of Mr. Worsencroft, and the workmen whom he employed.

*Turnham Green, November 27,* 1823.

### *Memoranda of the various strata bored through.*

Feet.

19 Gravel.

162 Blue clay. *Specimen No.* 1. At the depth of 59 feet from the surface a small stone, *sp. No.* 2, four inches thick was found; another at 149 feet was found six inches thick, but it was pounded to dust by the chisel; a third was found at the depth of 153 feet; it is marked *sp. No.* 3. At 162 feet from the surface the clay became veiny, and intermingled with very minute glittering fragments; this is *sp. No.* 4. At 173 feet the clay became more sandy, *sp. No.* 5, and continued so till it altered into the next kind.

30 Coloured clay; varying from brick-red, mixed with blue and yellow, to many shades of dull purple; *sp.* 6, came from 190 feet; *sp.* 7, from 200 feet; *sp.* 8, from 203 feet; *sp.* 9, from 211 feet, when the seam changes into the next which is more yellow.

22 Clay, with nearly an uniform colour of yellow ochre, occasionally mixed irregularly with grey. *Sp.* 10. This was more sandy than the previous stratum. Among this water rose in some quantity.

28.6 Soft soil, apparently composed of clay and sand. It varied very much in colour, being sometimes bright green, otherwise yellow intermixed with green, or sometimes beautifully veined with dark red and yellow. Many specimens are sent, *viz.*:

*sp.* 11, 240 ft. *sp.* 12, 242 ft. *sp.* 13, 243 ft.
*sp.* 14, 244 ft. *sp.* 15, 245 ft. *sp.* 16, 246 ft.
*sp.* 17, 247 ft. *sp.* 18, 255 ft. *sp.* 19, 261.6 ft.

The last specimen is of the soil immediately above the chalk. Two stones were met with in this stratum; one like those formerly mentioned, of which no specimen could be preserved; the other a flint, *sp.* 20, at 257 feet.

67.6 Chalk; among which many flints were scattered. Of these, one, *sp.* 21, was one foot in thickness, and so unusually hard as to occupy the workmen three days in punching before they could force a way through it. The water was found at the depth of 317 feet, in a bed of soft chalk, mixed with small flints; the hole was bored 12 feet among the water, so that the total depth of the well is 329 feet; and it is supposed by the workmen that the last piece of chalk that was brought up sticking to their punch, was from the upper surface of a new layer of chalk in which there is no water. *Specimen* 22, is a morsel of a hard stone, apparently containing ore, which was brought up in the auger from among the chalk, at the depth of 274 feet. *Specimen* 23, is of the first chalk which was found at 261.6 feet. *Specimen* 24, is from 317 feet, when the first water was found; it was saturated with moisture when first brought up; *sp.* 25, is the last piece of chalk brought from 329 feet, and supposed by the workmen to be from the upper surface of a new and dry layer of chalk; *sp.* 26, various fragments of large flints broken by the punch at different depths in the ground; *sp.* 27, morsels of flint and pebbles washed out of the chalk raised from the water-source, and supposed not to have been broken in punching, but to have laid among the water in their present condition. In cutting a solid piece of chalk, which had been brought up in the auger, a morsel of flint, exactly like these specimens, was observed, with every appearance of not having been forced into its place in the chalk by violence.

The principal impurity discovered in this water by the action of reagents is common salt, of which it contains about four grains and a half in the pint. When evaporated to dryness, the residue contains a sufficient quantity of carbonate of soda to render it very manifestly alcaline ; this is also the case with the waters of the other deep wells in and about London.

J. L.

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## Art. IX. *On the Taylorian Theorem.*

[To the Editor.]

Trinity College, Dublin,
*October* 11, 1823.

Sir,

I lately communicated to you a demonstration of the Taylorian Theorem given to me at lecture in this University by Mr. Edward Wilmot, a gentleman-commoner, and under-graduate of this college. The same gentleman has since given me an extension of this to functions of several variables, which I now enclose. The simplicity of the proof, and the elementary nature of its principles, must render it very valuable to the student. You will observe that it is independent of the Theorem of Maclaurin, and gives it as a corollary. It is also free from the functional reasoning so objectionable in other proofs of this theorem.

I am, Sir, *&c. &c.*,

Dionysius Lardner.

Let $u = F(x, x', x'', \ldots\ldots)$, $x, x', x''$, *&c.*, being independent variables,

Let $u' = F(x + \Delta x, x' + \Delta x', x'' + \Delta x'', \ldots\ldots)$

And let this be supposed to be expanded according to the dimensions of $x + \Delta x$, *&c.*

$$\begin{aligned} u' = {} & A(x + \Delta x) + A'(x' + \Delta x') + A''(x'' + \Delta x'') \ldots\ldots \\ & + A_2(x + \Delta x)^2 + A_2'(x' + \Delta x')^2 + A_2''(x'' + \Delta x'')^2 \ldots\ldots \\ & + B''(x + \Delta x)(x' + \Delta x') + B'(x + \Delta x)(x'' + \Delta x'') + B \\ & (x' + \Delta x')(x'' + \Delta x'') \ldots\ldots \\ & + A_3(x + \Delta x)^3 + A_3'(x' + \Delta x')^3 + A_3''(x'' + \Delta x'')^3 \ldots\ldots \\ & + \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \\ & \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \qquad (A) \end{aligned}$$

Hence

$$\begin{aligned} u = {} & Ax' + A'x' + A''x'' \;.\;.\;.\;.\;. \\ & A_2x^2 + A_2{}'x'^2 + A_2{}''x''^2 \;.\;.\;. \\ & B''xx' + B'xx'' + Bx'x'' \;.\;.\;. \\ & A_3x^3 + A_3{}'x'^3 + A_3{}''x''^3 \;.\;.\;. \\ & .\quad .\quad .\quad .\quad .\quad .\quad .\quad .\quad . \\ & .\quad .\quad .\quad .\quad .\quad .\quad .\quad .\quad . \qquad \text{(B)} \end{aligned}$$

This being successively differentiated for each of the variables, gives

$$\left\{\frac{du}{dx}\,\Delta x + \frac{du}{dx'}\,\Delta x' + \frac{du}{dx''}\,.\,\Delta x'' \;.\;.\;.\;.\;.\right\} =$$

$$\begin{aligned} = {} & \Delta x \,\{A + 2A_2x + B''x' + B'x'' + 3A_3x^2 \;.\;.\;.\;.\;.\} \\ + {} & \Delta x' \,\{A' + 2A_2{}'x' + B''x + Bx'' + 3A_3{}'x'^2 \;.\;.\;.\;.\} \\ + {} & \Delta x'' \,\{A'' + 2A_2{}''x'' + Bx' + B'x + 3A_3{}''x''^2 \;.\;.\;.\} \\ & .\quad .\quad .\quad .\quad .\quad .\quad .\quad .\quad . \\ & .\quad .\quad .\quad .\quad .\quad .\quad .\quad .\quad . \qquad \text{(C)} \end{aligned}$$

$$\left\{\frac{d^2u}{dx^2}\,.\,\frac{\Delta x^2}{1.2} + \frac{d^2u}{dx'^2}\,.\,\frac{\Delta x'^2}{1.2} + \frac{d^2u}{dx''^2}\,.\,\frac{\Delta x''^2}{1.2} \;.\;.\;.\;.\right\} =$$

$$\begin{aligned} = {} & \Delta x \,\{A_2 + 3A_3x \;.\;.\;.\;.\;.\;.\;.\;.\} \\ & \Delta x' \,\{A_2{}' + 3A_3{}'x \;.\;.\;.\;.\;.\;.\;.\;.\} \\ & \Delta x'' \,\{A_2{}'' + 3A''x \;.\;.\;.\;.\;.\;.\;.\;.\} \\ & .\quad .\quad .\quad .\quad .\quad .\quad . \\ & .\quad .\quad .\quad .\quad .\quad .\quad . \qquad \text{(D)} \end{aligned}$$

$$\left\{\frac{d^2u}{dx\;dx'}\,.\,\frac{\Delta x.\Delta x'}{1} + \frac{d^2u}{dx\;dx''}\,.\,\frac{\Delta x.\Delta x''}{1} + \frac{d^2u}{dx'\;dx''}\,.\,\frac{\Delta x'\;\Delta x''}{1} \ldots\right\} =$$

$$\begin{aligned} = {} & \Delta x\,.\,\Delta x' \,\{B'' \;.\;.\;.\;.\;.\;.\;.\} \\ + {} & \Delta x\;\Delta x'' \,\{B' \;.\;.\;.\;.\;.\;.\;.\} \\ + {} & \Delta x'\;\Delta x'' \,\{B \;.\;.\;.\;.\;.\;.\;.\} \\ & .\quad .\quad .\quad .\quad .\quad . \\ & .\quad .\quad .\quad .\quad . \qquad \text{(E)} \end{aligned}$$

The sum of the series (D) and (E) may evidently be expressed thus,

$$\frac{d^2u}{1.2}\left\{\frac{\Delta x}{dx} + \frac{\Delta x'}{dx'} + \frac{\Delta x''}{dx''} \;.\;.\;.\;.\right\}^2$$

And hence if the series (A) be arranged by the dimensions of the quantities, $\Delta x$, $\Delta x'$, $\Delta x''$, . . . . . and the substitutions suggested by the series (B), (C), (D), (E), &c., being made, the result will be

$$u' = u + \frac{du}{1} \cdot \text{S} \cdot \frac{\Delta x}{dx} \cdot + \frac{d^2u}{1.2} \cdot \left(\text{S}\ \frac{\Delta x}{dx}\right)^2 + \frac{d^3u}{1.2.3} \cdot \left(\text{S} \cdot \frac{\Delta x}{dx}\right)^3 \ldots\ldots\ldots\ldots$$

In which the symbol $\text{S}\ \frac{\Delta x}{dx}$ signifies

$$\frac{\Delta x}{dx} + \frac{\Delta x'}{dx'} + \frac{\Delta x''}{dx''} \ldots\ldots$$

The meaning of the symbols $\frac{d^2u}{1.2}\left(\text{S}.\frac{\Delta x}{dx}\right)^2$ &c., is sufficiently obvious.

This becomes identical with Taylor's series when $\text{S}\ \frac{\Delta x}{dx} = \frac{\Delta x}{dx}$.

The series of Maclaurin may easily be inferred from it by supposing $x = 0$ and changing $\Delta x$ into $x$.

## Art. X. ASTRONOMICAL PHENOMENA arranged in Order of Succession, for the Months of April, May, and June, in the Year 1824.

*(Continued from Page 297.)*

### APRIL.

| Days. | Planet's or Star's Name, &c. | Magnitude of Stars. | Sidereal Time. | Planet's or Star's Declination. |
|---|---|---|---|---|
| | | | H. M. | D. M. |
| 1 | Sun . . . . | | 0 43 | 4 38 N |
| | Mars . . . | | 12 11 | 2 8 N |
| | Juno . . . | | 14 18 | 2 19 S |
| | Venus . . | | 22 55 | 8 14 S |
| | Mercury . | | 0 13 | 0 46 S |
| 2 | Sun . . . | | 0 47 | 5 1 N |
| | Im. * . . . | 7.8 | 10 47 | or $10^{h}3'$ MT. |
| | *'s R.A. $3^{h}14'$ | | Decl. 21° 25′ N. (0′) | |
| | Em. . . . | | 11 33 | or $10^{h}49'$ MT. (4′N.) |
| | Mars . . . | | 12 10 | 2 16 N |
| | Juno . . . | | 14 17 | 2 11 S |
| | Venus . . | | 23 0 | 7 47 S |
| | Mercury . | | 0 20 | 0 5 N |
| 3 | Sun . . . . | | 0 50 | 5 24 N |
| | Im. * . . . | 7.8 | 9 40 | or $8^{h}51'$ MT. |
| | *'s R.A. $4^{h}9'$ | | Decl. 23° 36′ N. (4′S.) | |
| | Em. . . . | | 10 31 | or $9^{h}42'$ MT. (0′) |
| | Mars . . . | | 12 8 | 2 23 N |
| | Juno . . . | | 14 17 | 2 4 S |
| | Venus . . | | 23 4 | 7 20 S |
| | Mercury . | | 0 26 | 0 56 N |
| 4 | Sun . . . | | 0 54 | 5 47 N |
| | Em. 2 Sat. | | 11 48 | or $10^{h}55'$ MT. (98) |
| | Mars . . | | 12 7 | 2 29 N |
| | Juno . . . | | 14 16 | 1 56 S |
| | Venus . . | | 23 9 | 6 53 S |
| | Mercury . | | 0 33 | 1 47 N |
| 5 | Sun . . . . | | 0 58 | 6 10 N |
| | Moon . . . | | 6 0 | 24 11 N |
| | Im. * 1 . . | 1.8 | 7 52 | or $6^{h}56'$ MT. |
| | *'s R.A. $6^{h}3'$ | | Decl. 24° 2′ N. (7′S.) | |
| | Em. * 1 . | | 8 44 | or $7^{h}48'$ MT. (1′S) |
| | Im. * 2 . . | 7 | 8 47 | or $7^{h}51'$ MT. |
| | *'s R.A. $6^{h}6'$ | | Decl. 24° 1′ N. (2′S.) | |
| | Im. * 3 . . | 7 | 9 18 | or $8^{h}22'$ MT. |
| | *'s R.A. $6^{h}6'$ | | Decl. 23° 47′ N. (12′S ) | |
| | Em. * 2 . | | 9 45 | or $8^{h}49'$ MT. (5N) |
| | Em. * 3 . | | 10 1 | or $9^{h}5'$ MT. (7′ S) |
| | Im. * 4 . | 8 | 10 3 | or $9^{h}7'$ MT. |
| | *'s R.A. $6^{h}8'$ | | Decl. 23° 40′ N. (14′S.) | |
| | Im. * 5 . . | 7.8 | 10 13 | or $9^{h}17'$ MT. (9′S.) |
| | Em. * 4 . | | 10 36 | or $9^{h}40'$ MT. (11′S.) |
| | Em. * 5 . | | 10 52 | or $9^{h}56'$ MT. (9′S.) |
| | Im. * . 6 . | 8 | 11 20 | or $10^{h}24'$ MT. |
| | *'s R.A. $6^{h}11'$ | | Decl. 23° 50′ N. (5′N.) | |
| | Em. * 6 . | | 12 3 | or $11^{h}7'$ MT. (11′N) |
| | Mars . . . | | 12 5 | 2 36 N |
| | Im. Jupiter | | 12 28 | or $11^{h}32'$ MT. |
| | Im. * 7 . . | 7 | 12 46 | or $11^{h}49$ MT. |
| | *'s R.A. $6^{h}15'$ | | Decl. 23° 32′ N (2′S) | |
| | Im. * 8 . . | 7 | 12 51 | or $11^{h}54'$ MT. |
| | *'s R.A. $6^{h}15'$ | | Decl. 23° 25′ N. (1′S) | |
| | Im. * 9 . . | | 12 58 | or $12^{h}1'$ MT. |
| | *'s R.A. $6^{h}15'$ | | Decl. 23° 48′ N. (cont.) | |
| | Em. Jup$_{r}$. | | 12 22 | or $12^{h}25'$ MT. |
| | Em. * 7 . | | 13 35 | or $12^{h}38'$ MT. (4 N) |
| | Em. * 8 . | | 13 36 | or $12^{h}39'$ MT. (5′N) |
| | Juno . . . | | 14 15 | 1 49 S |
| | Venus . . | | 23 14 | 6 37 S |
| | Mercury . | | 0 40 | 2 40 N |
| 6 | Sun . . . . | | 1 1 | 6 33 N |
| | Moon . . . | | 7 3 | 22 6 N |
| | Mars . . . | | 12 4 | 2 42 N |
| | Juno . . . | | 14 15 | 1 41 S |
| | Venus . . | | 23 18 | 6° 0′ S |
| | Mercury . | | 0 48 | 3 34 N |
| 7 | Sun . . . . | | 1 5 | 6 55 N |
| | Moon . . . | | 8 4 | 18 30 N |
| | 20 Canc. . | 6 | 8 13 | 18 53 N |
| | θ . . . . . | 5·6 | 8 22 | 18 41 N |
| | VIII. 112 . | 8 | 8 28 | 19 53 N |
| | Im. * 1 . . | 7 | 8 40 | or 7 36 MT. |
| | *'s R.A. $8^{h}4'$ | | Decl. 18° 12′ N. (16′S.) | |
| | Em. * 1 . | | 9 13 | or $8^{h}9'$ MT. (11′S.) |
| | Im. * 2 . . | | 9 20 | or $8^{h}16'$ MT. |
| | *'s R.A. $8^{h}6'$ | | Decl. 18° 5′ N. (14′S.) | |
| | Em. 3 Sat. | | 9 38 | or $8^{h}34'$ MT. (+100) |
| | Em. * 2 . | | 10 7 | or $8^{h}16'$ MT. (5′S.) |
| | Em. 1 Sat. | | 11 15 | or $10^{h}11'$ MT. (+100) |
| | Mars . . . | | 12 3 | 2 48 N |
| | Juno . . . | | 14 14 | 1 34 S |
| | Venus . . | | 23 23 | 5 33 S |
| | Mercury . | | 0 55 | 4 27 N |
| 8 | Sun . . . . | | 1 9 | 7 18 N |
| | Moon . . . | | 9 3 | 13 38 N |
| | IX. 55 . . | 7.8 | 9 12 | 13 51 N |
| | Im. 4 Sat. | | 9 13 | or $8^{h}4'$ MT. (+100) |
| | IX. 84 . . | 7.8 | 9 17 | 15 4 N |
| | IX. 120 . . | 7·8 | 9 25 | 13 26 N |
| | Mars . . . | | 12 1 | 2 54 N |
| | Em. 4 Sat. | | 12 6 | or $10^{h}58'$ MT. (+101) |
| | Juno . . . | | 14 13 | 1 27 S |
| | Venus . . | | 23 27 | 5 5 S |
| | Mercury . | | 1 3 | 5 22 N |

## APRIL.

| Days. | Planet's or Star's Name, &c. | Magnitude of Stars. | Sidereal Time. | | Planet's or Star's Declination. | |
|---|---|---|---|---|---|---|
| | | | H. | M. | D. | M. |
| 9 | Sun . . . . | | 1 | 12 | 7 | 40 N |
| | IX. 202 . . | 8 | 9 | 45 | 8 | 54 N |
| | π Leonis . | 4·5 | 9 | 51 | 8 | 53 N |
| | Moon . . . | | 9 | 59 | 7 | 52 N |
| | 43 Leonis | 6 | 10 | 14 | 7 | 26 N |
| | Mars . . . | | 12 | 0 | 3 | 0 N |
| | Juno . . . | | 14 | 12 | 1 | 19 S |
| | Venus . . | | 23 | 32 | 4 | 38 S |
| | Mercury . | | 1 | 10 | 6 | 17 N |
| 10 | Sun . . . . | | 1 | 16 | 8 | 2 N |
| | Mercury . | | 1 | 17 | 7 | 12 N |
| | 55 Leonis | 6 | 10 | 47 | 1 | 40 N |
| | Moon . . . | | 10 | 54 | 1 | 35 N |
| | 65 Leonis. | 5.6 | 10 | 58 | 2 | 55 N |
| | 69 Leonis. | 5.6 | 11 | 5 | 0 | 53 N |
| | Mars . . . | | 11 | 59 | 3 | 5 N |
| | Juno . . . | | 14 | 12 | 1 | 12 S |
| | Im. * . . . | 6.7 | 16 | 33 or 15$^{h}$ 16′ MT. | | |
| | *'s R.A. 11$^{h}$ 6′ Decl. 0° 17′ S. (14′ S.) | | | | | |
| | Em. * . . | | 17 | 17 or 15$^{h}$ 59′ MT. (3′S.) | | |
| | Venus . . | | 23 | 36 | 4 | 10 S |
| 11 | Sun . . . . | | 1 | 20 | 8 | 24 N |
| | XI. 148 . . | 6·7 | 11 | 35 | 5 | 42 S |
| | XI. 167 . . | 6 | 11 | 42 | 4 | 21 S |
| | Moon . . . | | 11 | 48 | 4 | 49 S |
| | XI. 221 . . | 7.8 | 11 | 55 | 4 | 30 S |
| | Mars . . . | | 11 | 58 | 3 | 10 N |
| | Juno . . . | | 14 | 11 | 1 | 5 S |
| | Venus . . | | 23 | 41 | 3 | 42 S |
| 12 | Sun . . . . | | 1 | 23 | 8 | 46 N |
| | Mars . . . | | 11 | 57 | 3 | 15 N |
| | Moon . . . | | 12 | 43 | 10 | 58 S |
| | 49 Virg. | 5.6 | 12 | 59 | 9 | 48 S |
| | 52 Virg. . . | 8 | 13 | 2 | 9 | 10 S |
| | XIII. 25 . | 7.8 | 13 | 6 | 10 | 25 S |
| | Juno . . . | | 14 | 10 | 0 | 57 S |
| | Venus . . | | 23 | 45 | 3 | 15 S |
| 13 | Sun . . . . | | 1 | 27 | 9 | 8 N |
| | Mercury . | | 1 | 32 | 9 | 4 N |
| | Im. * . . . | 6.7 | 8 | 39 or 7$^{h}$ 11′ MT. | | |
| | *'s R.A. 13$^{h}$ 31′ Decl. 15° 33′ S. (16′S.) | | | | | |
| | Em. * . . | | 9 | 19 or 7$^{h}$ 51′ MT. (8′S.) | | |
| | Mars . . . | | 11 | 55 | 3 | 20 N |
| | 75 Virg. . | 6 | 13 | 23 | 14 | 27 S |
| | XIII. 139 | 8 | 13 | 28 | 15 | 33 S |
| | 83 Virg. . | 6 | 13 | 35 | 15 | 17 S |
| | Moon . . . | | 13 | 39 | 16 | 18 S |
| | Juno . . . | | 14 | 9 | 0 | 50 S |
| | Venus . . | | 23 | 50 | 2 | 47 S |
| 14 | Sun . . . . | | 1 | 31 | 9 | 30 N |
| | Mercury . | | 1 | 40 | 9 | 59 N |
| | Em. 3 Sat. | | 10 | 50 or 9$^{h}$ 18′ MT. (+100.) | | |
| | Mars . . . | | 11 | 54 | 3 | 24 N |
| | Juno . . . | | 14 | 9 | 0 | 43 S |
| | XIV. 116. | 7 | 14 | 25 | 19 | 49 S |
| | Moon . . . | | 14 | 36 | 20 | 42 S |
| | 10 Libræ . | 7 | 14 | 42 | 17 | 37 S |
| | Im. * . . . | 7 | 14 | 45 or 13$^{h}$ 12′ MT. | | |
| | *'s R.A. 14$^{h}$ 37′ Decl. 20° 35′S. (cont.) | | | | | |
| | XIV. 212. | 6 | 14 | 47 | 20 | 36 S |
| | Im. * . . . | 7.8 | 19 | 59 or 18$^{h}$ 25′ MT. | | |
| | *'s R.A. 14$^{h}$ 47′ Decl. 21° 26′N. (5′N.) | | | | | |
| | Em. * . . | | 20 | 48 or 19$^{h}$ 14′ MT. (10′N) | | |
| | Venus . . | | 23 | 54 | 2 | 19 S |
| 15 | Sun . . . . | | 1 | 34 | 9 | 51 N |
| | Mercury . | | 1 | 47 | 10 | 54 N |
| | Mars . . . | | 11 | 53 | 3 | 30 N |
| | Juno . . . | | 14 | 8 | 0 | 36 S |
| | Im. * . . . | 6 | 14 | 16 or 12$^{h}$ 40′ MT. | | |
| | *'s R.A. 15$^{h}$ 34′ Decl. 23° 50′ N. (2′S.) | | | | | |
| | Em. * . . | | 15 | 24 or 13$^{h}$ 47′ MT. (6′N.) | | |
| | 42 Libræ . | 5.6 | 15 | 30 | 23 | 14 S |
| | Moon . . . | | 15 | 35 | 23 | 50 S |
| | XV. 192 . | 6 | 15 | 43 | 23 | 27 S |
| | XV. 213 . | 7.8 | 15 | 48 | 23 | 1 S |
| | Im. * . . . | 6 | 20 | 4 or 18$^{h}$ 27′ MT. | | |
| | *'s R.A. 15$^{h}$ 43′ Decl. 24° 0′ N. (cont.) | | | | | |
| | Venus . . | | 23 | 59 | 1 | 51 S |
| 16 | Sun . . . . | | 1 | 38 | 10 | 12 N |
| | Mercury . | | 1 | 55 | 11 | 50 N |
| | Mars . . . | | 11 | 52 | 3 | 31 N |
| | Im. * . . . | 7.8 | 13 | 49 or 12$^{h}$ 9′ (MT) | | |
| | *'s R.A. 16$^{h}$ 29′ Decl. 25° 42′ S. (15′S.) | | | | | |
| | Em. * . . | | 14 | 2 or 12$^{h}$ 22′ MT. (14′S.) | | |
| | Juno . . . | | 14 | 7 | 0 | 29 S |
| | Venus . . | | 0 | 3 | 1 | 23 S |
| 17 | Sun . . . . | | 1 | 42 | 10 | 33 N |
| | Mercury . | | 2 | 3 | 12 | 43 N |
| | Mars . . . | | 11 | 51 | 3 | 35 N |
| | Juno . . . | | 14 | 6 | 0 | 22 S |
| | Venus . . | | 0 | 8 | 0 | 54 S |
| 18 | Sun . . . . | | 1 | 45 | 10 | 54 N |
| | Mercury . | | 2 | 11 | 13 | 37 N |
| | Mars . . . | | 11 | 50 | 3 | 38 N |
| | Im. * . . . | 7.8 | 13 | 21 or 11$^{h}$ 33′ MT. | | |
| | *'s R.A. 18$^{h}$ 20′ Decl. 25° 0′ N. (5′S.) | | | | | |
| | Juno . . . | | 14 | 5 | 0 | 15 S |
| | Em. * . . | | 14 | 18 or 12$^{h}$ 30′ MT. (5′S.) | | |
| | Venus . . | | 0 | 12 | 0 | 26 S |
| 19 | Sun . . . . | | 1 | 49 | 11 | 15 N |
| | Mercury . | | 2 | 19 | 14 | 40 N |
| | Mars . . . | | 11 | 50 | 3 | 40 N |
| | Juno . . . | | 14 | 5 | 0 | 8 S |
| | Im. * 1 . . | 7.8 | 14 | 19 or 12$^{h}$ 27′ MT. | | |

## APRIL.

| Days. | Planet's or Star's Name, &c. | Magnitude of Stars | Sidereal Time. H. M. | Planet's or Star's Declination. D. M. |
|---|---|---|---|---|
| | *'s R.A. 19$^{h}$ 14′ Decl. 22° 54′S. (4′S.) | | | |
| | Im. * 2 . . | 7.8 | 14 51 | or 12$^{h}$ 59′ MT. |
| | *'s R.A. 19$^{h}$ 15′ Decl. 22° 47′ S. (1′N.) | | | |
| | Em. * 1 . | | 15 14 | or 13$^{h}$ 22′ MT. (8′S.) |
| | Em. * 2 . | | 15 57 | or 14$^{h}$ 5′ MT. 3′S.) |
| | Venus . . | | 0 17 | 0 2 N |
| 20 | Sun . . . . | | 1 53 | 11 36 N |
| | Mercury . | | 2 27 | 15 19 N |
| | Mars . . . | | 11 49 | 3 43 N |
| | Juno . . . | | 14 4 | 0 2 S |
| | Im. * 1 . . | 8 | 14 53 | or 12$^{h}$ 57′ MT. |
| | *'s R.A. 20$^{h}$ 5′ Decl. 19° 44′ S. (5′N.) | | | |
| | Em. * 1 . | | 15 54 | or 13$^{h}$ 58′ MT. (2′S.) |
| | Im. * 2 . . | 8 | 16 0 | or 14$^{h}$ 4′ MT. |
| | *'s R.A. 20$^{h}$ 6′ Decl. 19° 26′S. (cont.) | | | |
| | Venus . . | | 0 21 | 0 30 N |
| 21 | Sun . . . . | | 1 57 | 11 56 N |
| | Mercury . | | 2 35 | 16 8 N |
| | Mars . . . | | 11 48 | 3 45 N |
| | Juno . . . | | 14 3 | 0 4 N |
| | Venus . . | | 0 26 | 0 59 N |
| 22 | Sun . . . . | | 2 0 | 12 16 N |
| | Mercury . | | 2 43 | 16 57 N |
| | Mars . . . | | 11 47 | 3 47 N |
| | Juno . . . | | 14 2 | 0 11 N |
| | Venus . . | | 0 30 | 1 27 N |
| 23 | Sun . . . . | | 2 4 | 12 36 N |
| | Mercury . | | 2 51 | 17 41 N |
| | Em. 1 Sat. | | 10 38 | or 8$^{h}$ 31′ MT. (+100) |
| | Mars . . . | | 11 47 | 3 48 N |
| | Juno . . . | | 14 1 | 0 17 N |
| | Venus . . | | 0 34 | 1 56 N |
| 24 | Sun . . . . | | 2 8 | 12 56 N |
| | Mercury . | | 2 59 | 18 24 N |
| | Mars . . | | 11 46 | 3 49 N |
| | Juno . . . | | 14 1 | 0 23 N |
| | Venus . . | | 0 39 | 2 24 N |
| 25 | Sun . . . . | | 2 12 | 13 16 N |
| | Mercury . | | 3 7 | 19 8 N |
| | Mars . . . | | 11 45 | 3 50 N |
| | Juno . . . | | 13 0 | 0 29 N |
| | Venus . . | | 0 43 | 2 53 N |
| 26 | Sun . . . . | | 2 15 | 13 35 N |
| | Mercury . | | 3 14 | 19 44 N |
| | Mars . . . | | 11 45 | 3 51 N |
| | Juno . . . | | 13 59 | 0 35 N |
| | Venus . . | | 0 47 | 3 21 N |
| 27 | Sun . . . . | | 2 19 | 13 54 N |
| | Mercury . | | 3 21 | 20 20 N |
| | Mars . . . | | 11 44 | 3 51 N |
| | Juno . . . | | 13 58 | 0 41 N |
| | Venus . . | | 0 52 | 3 50 N |
| 28 | Sun . . . . | | 2 23 | 14 13 N |
| | Mercury . | | 3 28 | 20 56 N |
| | Mars . . . | | 11 44 | 3 51 N |
| | Juno . . . | | 13 58 | 0 47 N |
| | Venus . . | | 0 56 | 4 18 N |
| 29 | Sun . . . . | | 2 27 | 14 32 N |
| | Mercury . | | 3 36 | 21 27 N |
| | Mars . . . | | 11 44 | 3 51 N |
| | Juno . . . | | 13 57 | 0 53 N |
| | Venus . . | | 1 1 | 4 46 N |
| 30 | Sun . . . . | | 2 31 | 14 51 N |
| | Mercury . | | 3 43 | 21 57 N |
| | Im. * 1 . . | 7.8 | 9 59 | or 7$^{h}$ 24′ MT. |
| | *'s R.A. 3$^{h}$ 49′ Decl. 23° 7′ N. (cont.) | | | |
| | Im. * 2 . . | 7.8 | 10 22 | or 7$^{h}$ 47′ MT. |
| | *'s R.A. 3$^{h}$ 50′ Decl. 22° 42′ N. (3′ S.) | | | |
| | Em. * 2 . | | 11 8 | or 8$^{h}$ 33′ MT. (4′S.) |
| | Mars . . . | | 11 43 | 3 50 N |
| | Em. 1 Sat. | | 13 1 | or 10$^{h}$ 26′ MT. (+100) |
| | Juno . . . | | 13 56 | 0 58 N |
| | Venus . . | | 1 5 | 5 15 N |

## MAY.

| Days. | Planet's or Star's Name, &c. | Magnitude of Stars. | Sidereal Time. | Planet's or Star's Declination. |
|---|---|---|---|---|
| | | | H. M. | D. M. |
| 1 | Sun . . . . | | 2 34 | 15 9 N |
| | Mercury . | | 3 49 | 22 23 N |
| | Mars . . . | | 11 43 | 3 49 N |
| | Im. * . . . | 6 | 13 3 or 10$^h$ 24' MT. | |
| | *'s R.A. 4$^h$ 57' | | Decl. 24° 1'N. (1'S.) | |
| | Em. . . . | | 13 47 or 11$^h$ 8' MT. (0'.) | |
| | Venus . . | | 1 10 | 5 43 N |
| 2 | Sun . . . . | | 2 38 | 15 27 N |
| | Mercury . | | 3 55 | 22 45 N |
| | Mars . . . | | 11 43 | 3 48 N |
| | Im. * 1 . . | 8 | 12 8 or 9$^h$ 25' MT. | |
| | *'s R.A. 5$^h$ 55' | | Decl. 23° 39' N. (12'S.) | |
| | Im. * 2 . . | | 12 23 or 9$^h$ 40' MT. | |
| | *'s R.A. 5$^h$ 56' | | Decl. 23° 39' N. (11'S.) | |
| | Em. * 1 . | | 12 42 or 9$^h$ 59' MT. (8'S.) | |
| | Em. * 2 . | | 13 2 or 10$^h$ 19' MT. (6'S.) | |
| | Venus . . | | 1 14 | 6 10 N |
| 3 | Sun . . . . | | 2 42 | 15 41 N |
| | Mercury . | | 4 1 | 23 6 N |
| | Mars . . . | | 11 43 | 3 46 N |
| | Im. * . . . | 7 | 13 34 or 10$^h$ 47' MT. | |
| | *'s R.A. 6$^h$ 0' | | Decl. 21° 33' N. (9'S.) | |
| | Em. . . . | | 14 21 or 11$^h$ 34' MT. (1'S.) | |
| | Venus . . | | 1 19 | 6 38 N |
| 4 | Sun . . . . | | 2 46 | 16 2 N |
| | Mercury . | | 4 7 | 23 28 N |
| | Im. * 1 . . | 7.8 | 10 35 or 7$^h$ 45' MT. | |
| | *'s R.A. 7$^h$ 51' | | Decl. 18° 43' N. (cont.) | |
| | Im. * 2 . . | 7 | 10 35 or 7$^h$ 45' MT. | |
| | *'s R.A. 7$^h$ 52' | | Decl. 18° 43' N. (15'S.) | |
| | Em. * 2 . | | 11 19 or 8$^h$ 29' MT. (7'N) | |
| | Mars . . . | | 11 42 | 3 44 N |
| | Im. * 3 . . | 6 | 15 7 or 12$^h$ 16' MT. | |
| | *'s R.A. 8$^h$ 2' | | Decl. 18° 10' N. (9'N.) | |
| | Em. * 3 . | | 15 38 or 12$^h$ 47' MT. (16'N.) | |
| | Venus . . | | 1 23 | 7 5 N |
| 5 | Sun . . . . | | 2 50 | 16 19 N |
| | Mercury . | | 4 13 | 23 42 N |
| | Moon . . . | | 8 45 | 14 59 N |
| | Mars . . . | | 11 42 | 3 42 N |
| | Venus . . | | 1 28 | 7 33 N |
| 6 | Sun . . . . | | 2 50 | 16 19 N |
| | Mercury . | | 4 13 | 23 42 N |
| | Moon . . . | | 9 41 | 9 31 N |
| | Im. * 1 . . | 8 | 11 35 or 8$^h$ 36' MT. | |
| | *'s R.A. 9$^h$ 45' | | Decl. 8° 54' N (13'S ) | |
| | Mars . . . | | 11 42 | 3 39 N |
| | Em. * 1 . | | 12 41 or 9$^h$ 42' MT. (3'N.) | |
| | Im. * 2 . . | | 12 57 or 9$^h$ 58' MT. | |
| | *'s R.A. 9$^h$ 47' | | Decl. 8° 30' N. (16'S.) | |
| | Em. 2 Sat. | | 13 35 or 10$^h$ 36' MT (+100.) | |
| | Em. * 2 . | | 13 39 or 10$^h$ 40' MT. (15'S.) | |
| | Venus . . | | 1 32 | 8 0 N |
| 7 | Sun . . . . | | 2 57 | 16 53 N |
| | Mercury . | | 4 24 | 24 11 N |
| | Moon . . . | | 10 36 | 3 35 N |
| | Im * . . . | 6 | 10 37 or 7$^h$ 34' MT. | |
| | *'s R.A. 10$^h$ 36' | | Decl. 3° 15' N. (7'S.) | |
| | X. 172 . . | 8 | 10 42 | 4 31 N |
| | 58 Leonis | 5 | 10 51 | 4 34 N |
| | X. 229 . . | | 10 55 | 4 35 N |
| | Mars . . . | | 11 42 | 3 37 N |
| | Em. * . . | | 11 49 or 8$^h$ 46' MT (11'N.) | |
| | Venus . . | | 1 37 | 8 28 N |
| 8 | Sun . . . . | | 3 1 | 17 9 N |
| | Mercury . | | 4 8 | 21 19 N |
| | 87 Leonis | 4.5 | 11 21 | 2 2 S |
| | Moon . . . | | 11 28 | 2 36 S |
| | Mars . . . | | 11 42 | 3 34 N |
| | XI. 179 . . | 8 | 11 45 | 2 48 S |
| | XI. 213 . . | 7 | 11 52 | 0 47 S |
| | Venus . . | | 1 42 | 8 55 N |
| 9 | Sun . . . . | | 3 5 | 17 25 N |
| | Mercury . | | 4 33 | 24 27 N |
| | Mars . . | | 11 42 | 3 30 N |
| | 14 Virg. . | 6.7 | 12 10 | 7 56 S |
| | Moon . . . | | 12 21 | 8 39 S |
| | 22 Virg. . | 5.6 | 12 25 | 8 29 S |
| | 31 d. l. . . | 6 | 12 33 | 8 2 S |
| | Im. * . . . | 7 | 16 48 or 13$^h$ 37' MT. | |
| | *'s R.A, 12$^h$ 1' | | Decl, 2° (11'S.) | |
| | Em. . . | | 17 49 or 14$^h$ 37' MT. (1'N.) | |
| | Venus . . | | 1 46 | 9 21 N |
| 10 | Sun . . . . | | 3 9 | 17 41 N |
| | Mercury . | | 4 37 | 24 35 N |
| | Mars . . . | | 11 43 | 3 27 N |
| | XIII. 19 . | 7·8 | 13 4 | 12 32 S |
| | Moon . . | | 13 15 | 14 10 S |
| | 75 Virg. . | 6 | 13 23 | 14 27 S |
| | XIII. 177 . | 7.8 | 13 35 | 13 20 S |
| | Venus . . | | 1 51 | 9 48 S |
| 11 | Sun . . . . | | 3 13 | 17 56 N |
| | Mercury . | | 4 41 | 24 37 N |
| | Mars . . . | | 11 43 | 3 23 N |
| | XIV. 22 . | 6 | 14 6 | 17 23 S |
| | Moon . . . | | 14 11 | 18 56 S |
| | XIV. 116 . | 7 | 14 25 | 19 40 S |
| | 10 Libræ . | 7 | 14 42 | 17 37 S |
| | Im. * 1 . . | 7.8 | 16 28 or 13$^h$ 9' MT. | |
| | *'s R.A. 14$^h$ 16' | | Decl. 19° 10' S. (12'N.) | |
| | Im. * 2 . . | 7 | 16 30 or 13$^h$ 11' MT. | |
| | *'s R.A. 14$^h$ 16' | | Decl. 19° (10'S.) (12'N.) | |
| | Em. * 1 . | | 16 52 or 13$^h$ 33' MT. (16'N.) | |
| | Em. * 2 . | | 16 52 or 13$^h$ 33' MT. (15'N.) | |

## MAY.

| Days. | Planet's or Star's Name, &c. | Magnitude of Stars. | Sidereal Time. | Planet's or Star's Declination. |
|---|---|---|---|---|
| | | | H. M. | D. M. |
| | Venus | | 1 56 | 10 15 N |
| 12 | Sun | | 3 17 | 18 12 N |
| | Mercury | | 4 45 | 24 39 N |
| | Mars | | 11 43 | 3 19 N |
| | XIV. 262 | 7 | 14 56 | 22 38 S |
| | XIV. 282 | 6 | 15 0 | 23 18 S |
| | Moon | | 15 9 | 22 34 S |
| | XV. 65 | | 15 16 | 20 45 S |
| | Venus | | 2 0 | 10 41 N |
| 13 | Sun | | 3 21 | 18 27 N |
| | Mercury | | 4 48 | 24 41 N |
| | Mars | | 11 43 | 3 15 N |
| | Im. * 1 | 7.8 | 14 16 or 10$^{h}$ 49′ MT. | |
| | *'s R.A. 16$^{h}$ 6′ Decl. 25° 0′ S. (15′S.) | | | |
| | Im. * 2 | 7 | 14 20 or 10$^{h}$ 53′ MT. | |
| | *'s R.A. 16$^{h}$ 4′ Decl. 25° 1′ N. (cont.) | | | |
| | Em. * 1 | | 14 37 or 11$^{h}$ 10′ MT. (13′S.) | |
| | Moon | | 16 7 | 24 53 S |
| | ο Scorpii | 5.6 | 16 10 | 23 44 S |
| | 5 | 5 | 16 15 | 23 2 S |
| | 22 | 6 | 16 20 | 24 43 S |
| | Im. * | 4 | 17 29 or 14$^{h}$ 2′ MT. | |
| | *'s R.A. 16$^{h}$ 10′ Decl. 25° 10′ S. (10′S.) | | | |
| | Em. * | | 18 25 or 14$^{h}$ 58′ MT. (7′S.) | |
| | Venus | | 2 5 | 11 8 N |
| 14 | Sun | | 3 25 | 18 41 N |
| | Mercury | | 4 51 | 24 38 N |
| | Mars | | 11 44 | 3 10 N |
| | XVI. 248 | 6 | 16 49 | 24 49 S |
| | 28 Oph. | 7 | 16 53 | 25 26 S |
| | Moon | | 17 6 | 25 43 S |
| | θ Oph. | 3.4 | 17 11 | 24 49 S |
| | Venus | | 2 10 | 11 33 N |
| 15 | Sun | | 3 29 | 18 55 N |
| | Mercury | | 4 54 | 24 34 N |
| | Mars | | 11 44 | 3 5 N |
| | Im. * 1 | 7 | 14 18 or 10$^{h}$ 44′ MT. | |
| | *'s R.A. 17$^{h}$ 58′ Decl. 25° 29′ S. (13′S.) | | | |
| | Im. * 2 | 7 | 14 24 or 10$^{h}$ 50′ MT. | |
| | *'s R.A. 17$^{h}$ 58′ Decl. 25° 29′ S. (13′S.) | | | |
| | Im. * 3 | 6 | 14 24 or 10$^{h}$ 50′ MT. | |
| | Em. * 1 | | 14 56 or 11$^{h}$ 21′ MT. 513′s.) | |
| | *'s R.A. 17$^{h}$ 58′ Decl. 25° 29′ S. (13′S.) | | | |
| | Em. * 2 & 3 | | 15 1 or 11$^{h}$ 26′ MT. (13′S.) | |
| | 63 Oph. | 6.7 | 17 44 | 24 51 S |
| | 4 Sagit. | 5 | 17 49 | 23 47 S |
| | XVII. 342 | 7 | 17 54 | 24 24 S |
| | Moon | | 18 3 | 25 10 S |
| | Venus | | 2 14 | 11 58 N |
| 16 | Sun | | 3 33 | 19 9 N |
| | Mercury | | 4 57 | 24 31 N |
| | Mars | | 11 44 | 3 0 N |
| | Im. * 1 | 8.9 | 13 54 or 10$^{h}$ 16′ MT. | |
| | *'s R.A. 18$^{h}$ 51′ Decl. 23° 28′ S. (13′N.) | | | |
| | Em. * 1 | | 14 29 or 10$^{h}$ 51 MT. (12′N.) | |
| | Im. * 2 | | 17 51 or 14$^{h}$ 12′ MT. | |
| | *'s R.A. 18$^{h}$ 58′ Decl. 23° 27′ S. (1′S.) | | | |
| | Im. * 3 | | 18 25 or 14$^{h}$ 46′ MT | |
| | *'s R.A. 18$^{h}$ 59′ Decl. 23° 28′ S. (4′S.) | | | |
| | Em. * 2 | | 19 9 or 15$^{h}$ 30′ MT. (1′S.) | |
| | Em. * 3 | | 19 33 or 15$^{h}$ 54′ MT. (9′S.) | |
| | Venus | | 2 19 | 12 23 N |
| 17 | Sun | | 3 37 | 19 23 N |
| | Mercury | | 4 59 | 24 22 N |
| | Mars | | 11 45 | 2 55 N |
| | Im. * | 8 | 18 57 or 15$^{h}$ 14′ MT. | |
| | *'s R.A. 19$^{h}$ 50′ Decl. 20° 20′ S. (4′S.) | | | |
| | Em. * | | 19 53 or 16 10 MT. (9′S.) | |
| | Venus | | 2 24 | 12 48 N |
| 18 | Sun | | 3 41 | 19 36 N |
| | Mars | | 5 1 | 24 14 N |
| | Mercury | | 11 45 | 2 49 N |
| | Venus | | 2 28 | 13 13 N |
| 19 | Sun | | 3 45 | 19 49 N |
| | Mercury | | 5 2 | 24 5 N |
| | Mars | | 11 46 | 2 44 N |
| | Im. * | | 17 4 or 13$^{h}$ 13′ MT. | |
| | *'s R.A. 21$^{h}$ 23′ Decl. 13° 2′ S.) 1′N.) | | | |
| | Em. * | | 18 7 or 14$^{h}$ 16′ MT. (10′S.) | |
| | Venus | | 2 33 | 13 38 |
| 20 | Sun | | 3 49 | 20 2 N |
| | Mercury | | 5 2 | 23 52 N |
| | Mars | | 11 46 | 2 38 N |
| | Venus | | 2 38 | 14 1 N |
| 21 | Sun | | 3 53 | 20 14 N |
| | Mercury | | 5 3 | 23 40 |
| | Mars | | 11 47 | 2 31 N |
| | Im. * | 8.9 | 17 30 or 13$^{h}$ 31′ MT. | |
| | *'s R.A. 22$^{h}$ 51′ Decl. 3° 23′ S. (1′N.) | | | |
| | Em. | | 18 28 or 14$^{h}$ 29′ MT. (10′S.) | |
| | Venus | | 2 43 | 14 24 N |
| 22 | Sun | | 3 57 | 20 26 N |
| | Mercury | | 5 3 | 23 27 N |
| | Mars | | 11 47 | 2 25 N |
| | Venus | | 2 47 | 14 47 N |
| 23 | Sun | | 4 1 | 20 38 N |
| | Mercury | | 5 3 | 23 11 N |
| | Mars | | 11 48 | 2 18 N |
| | Venus | | 2 52 | 15 11 N |
| 24 | Sun | | 4 5 | 20 49 N |
| | Mercury | | 5 2 | 22 54 N |
| | Mars | | 11 49 | 2 12 N |
| | Im. * | 7.8 | 19 24 or 15$^{h}$ 13′ MT. | |
| | *'s R.A. 1$^{h}$ 9′ Decl. 12° 12′ N. (14′N.) | | | |

## MAY.

| Days. | Planet's or Star's Name, &c. | Magnitude of Stars. | Sidereal Time. | Planet's or Star's Declination. |
|---|---|---|---|---|
| | | | H. M. | D. M. |
| | Em. * . . | | 19 33 or 15$^h$22′ MT. (13′N.) | |
| | Venus . . | | 2 57 | 15 34 N |
| 25 | Sun . . . . | | 4 9 | 21 0 N |
| | Mercury . | | 5 2 | 22 38 N |
| | Mars . . . | | 11 50 | 2 5 N |
| | Im. * . . . | 7 | 18 59 or 14$^h$ 45′ MT. | |
| | *'s R.A. 2$^h$ 0′ | | | Decl. 16° 24′ N. (8′N.) |
| | Em. * . . . | | 19 50 or 15$^h$ 36′ MT. (2′N.) | |
| | Venus . . | | 3 2 | 15 57 N |
| 26 | Sun . . . . | | 4 13 | 21 10 N |
| | Mercury . | | 5 1 | 22 19 N |
| | Mars . . . | | 11 50 | 1 57 N |
| | Venus . . | | 3 7 | 16 18 N |
| 27 | Sun . . . . | | 4 17 | 21 20 N |
| | Mercury . | | 5 0 | 22 0 N |
| | Mars . . . | | 11 51 | 1 50 N |
| | Im. 3 Sat. | | 13 40 or 9$^h$ 19′ MT. (+100) | |
| 28 | Sun . . . . | | 4 21 | 21 30 N |

| Days. | Planet's or Star's Name, &c. | Magnitude of Stars. | Sidereal Time. | Planet's or Star's Declination. |
|---|---|---|---|---|
| | | | H. M. | D. M. |
| | Mercury . | | 4 59 | 21 42 N |
| | Mars . . . | | 11 52 | 1 42 N |
| | Venus. . . | | 3 17 | 16 59 N |
| 29 | Sun . . . . | | 4 25 | 21 40 N |
| | Mercury . | | 4 57 | 21 22 N |
| | Mars . . . | | 11 53 | 1 34 N |
| | Venus . , | | 3 22 | 17 19 N |
| 30 | Sun . . . . | | 4 29 | 21 49 N |
| | Mercury . | | 4 55 | 21 2 N |
| | Mars . . . | | 11 54 | 1 27 N |
| | Im. * . . . | 6.7 | 14 37 or 10$^h$ 3′ MT. | |
| | *'s R.A. 6$^h$ 41′ | | | Decl. 22° 0′ N. (6′S.) |
| | Em. * . . | | 15 20 or 10$^h$ 46′ MT. (4′N.) | |
| | Venus . . | | 3 27 | 17 40 N |
| 31 | Sun . . . . | | 4 33 | 21 57 N |
| | Mercury . | | 4 53 | 20 42 N |
| | Mars . . . | | 11 55 | 1 18 N |
| | Venus . . | | 3 32 | 18 0 N |

## JUNE.

| | | | | |
|---|---|---|---|---|
| | | | H. M. | D. M. |
| 1 | Sun . . . . | | 4 37 | 22 5 N |
| | Mercury . | | 4 51 | 20 21 N |
| | Georgian . | | 19 6 | 23 1 S |
| | Venus . . | | 3 37 | 18 21 N |
| 2 | Sun . . . . | | 4 41 | 22 14 N |
| | Mercury . | | 4 49 | 20 1 N |
| | Im. * . . . | 7 | 15 56 or 11$^h$ 11′ MT. | |
| | *'s R.A. 9$^h$ 36′ | | | Decl. 9° 41′ N. (5′N.) |
| | Em . . . . | | 16 35 or 11$^h$50′ MT. (14′N) | |
| | Georgian . | | 19 6 | 23 1 S |
| | Venus . . | | 3 42 | 18 38 N |
| 3 | Sun . . . . | | 4 45 | 22 21 N |
| | Moon . . . | | 10 18 | 5 15 N |
| | Im. * . . . | 6.7 | 15 24 or 10$^h$ 35′ MT. | |
| | *'s R.A. 10$^h$ 27′ | | | Decl. 4° 27′ S. (cont.) |
| | Georgian . | | 19 6 | 23 1 S |
| | Venus - . | | 3 47 | 18 56 N |
| | Mercury . | | 4 44 | 19 22 N |
| 4 | Sun . . . . | | 4 49 | 22 28 N |
| | Moon . . . | | 11 11 | 0 53 S |
| | Im. * 1 . . | 7.8 | 15 40 or 10 47 MT. | |
| | *'s R.A. 11$^h$ 20′ | | | Decl. 2° 1′ S. (8′N.) |
| | Im. * 2 . . | 4.5 | 16 11 or 11$^h$ 18′ MT. | |
| | *'s R.A. 11$^h$ 21′ | | | Decl. 2° 2′ S. (1′S.) |
| | Em. * 1 . | | 16 40 or 11$^h$ 47′ MT. (7′S.) | |
| | Em. * 2 . | | 17 6 or 12$^h$13′ MT. (13′S.) | |
| | Georgian . | | 19 5 | 23 2 S |
| | Venus . . | | 3 52 | 19 13 N |

| | | | | |
|---|---|---|---|---|
| | | | H. M. | D. M. |
| | Mercury . | | 4 42 | 19 5 N |
| 5 | Sun . . . . | | 4 54 | 22 35 N |
| | Moon . . . | | 12 5 | 6 55 S |
| | Im. * 1 . . | 7.8 | 14 39 or 9$^h$ 42′ MT. | |
| | *'s R.A. 12$^h$ 8′ | | | Decl. 7° 48′ S. (15′S.) |
| | Im. * 2 . . | 8 | 15 13 or 10$^h$ 16′ MT. | |
| | *'s R.A. 12$^h$ 9′ | | | Decl. 7° 55′S. (16′S.) |
| | Em. * 1 . | | 15 15 or 10$^h$ 18′ MT. (8′S.) | |
| | Im. * 3 . | | 15 31 or 10$^h$ 34′ MT. | |
| | *'s R.A. 12$^h$ 10′ | | | Decl. 7° 56′S. (14′S.) |
| | Em. * 3 . | | 15 49 or 10$^h$ 52′ MT. (5′S. | |
| | Em. * 2 . | | 15 51 or 10$^h$54′ MT. (10′S. | |
| | Georgian . | | 19 5 | 23 2 S |
| | Venus . . | | 3 57 | 19 31 N |
| | Mercury. . | | 4 40 | 18 50 N |
| 6 | Sun . . . . | | 4 57 | 22 41 N |
| | Moon . . . | | 12 57 | 12 32 S |
| | Georgian . | | 19 5 | 23 2 S |
| | Venus . . | | 4 2 | 19 48 N |
| | Mercury . | | 4 39 | 18 35 N |
| 7 | Sun . . . . | | 5 2 | 22 47 N |
| | Moon . . . | | 13 51 | 17 28 S |
| | XIII. 317 . | 6 | 14 1 | 15 28 S |
| | XIV. 22 . | 6 | 14 6 | 17 23 S |
| | XIV. 38 . | 7.8 | 14 10 | 17 42 S |
| | Georgian . | | 19 5 | 23 2 S |
| | Venus . . | | 4 7 | 20 6 N |
| | Mercury . | | 4 37 | 18 20 N |

## JUNE.

| Days. | Planet's or Star's Name, &c. | Magnitude of Stars. | Sidereal Time. | Planet's or Star's Declination. |
|---|---|---|---|---|
| | | | H. M. | D. M. |
| 8 | Sun . . . . | | 5 6 | 22 53 N |
| | Im. * 1 . . | | 14 13 | or 9$^{h}$ 5′ MT. |
| | *'s R.A. 14$^{h}$ 47′ Decl. 21° 26′ S. (2′S.) | | | |
| | XIV. 171 . | 7 | 14 37 | 20 35 S |
| | Moon . . . | | 14 47 | 21 24 S |
| | XV. 19 . . | 6.7 | 15 6 | 21 44 S |
| | Im. * 2 . . | 7.8 | 15 8 | or 9$^{h}$ 59′ MT. |
| | *'s R.A. 14$^{h}$ 49′ Decl. 21° 41′ S. (10′S.) | | | |
| | XV. 65 . . | 8 | 15 16 | 20 45 S |
| | Em. * 1 . | | 15 25 | or 10$^{h}$ 16′ MT. (8′S.) |
| | Em. * 2 . | | 16 14 | or 11$^{h}$ 5′ MT. (6′S.) |
| | Georgian . | | 19 5 | 23 2 |
| | Venus . . | | 4 12 | 20 20 N |
| | Mercury . | | 4 35 | 18 9 N |
| 9 | Sun . . . . | | 5 10 | 22 58 N |
| | Im. * 1 . . | 6 | 14 29 | or 9$^{h}$ 16′ MT. |
| | *'s R.A. 15$^{h}$ 43′ Decl. 24° 0′ S. (6′N.) | | | |
| | Im. * 2 . . | | 14 45 | or 9$^{h}$ 32′ MT. |
| | *'s R.A. 15$^{h}$ 44′ Decl. 24° 3′ S. (5′N.) | | | |
| | Em. * 1 . | | 15 23 | or 10$^{h}$ 10′ MT. (12′N.) |
| | 42 Libræ . | 5.6 | 15 30 | 23 14 S |
| | XV. 149 . | 7.8 | 15 33 | 24 51 S |
| | Moon . . | | 15 45 | 24 9 S |
| | Em. * 2 . | | 15 47 | or 10$^{h}$ 34′ MT. (11′N.) |
| | XV. 225 . | 3 | 15 50 | 22 7 S. |
| | Im * 3 . . | 6.7 | 17 4 | or 11$^{h}$ 51′ MT. |
| | *'s R.A. 15$^{h}$ 48′ Decl. 24° 20′ S. (1′S.) | | | |
| | Em. * 3 . | | 18 15 | or 13$^{h}$ 2′ MT. (3′N.) |
| | Georgian . | | 19 4 | 23 3 S |
| | Im. * 4 . . | 7 | 19 27 | or 14$^{h}$ 14′ MT. |
| | *'s R.A. 15$^{h}$ 52′ Decl. 24° 31′ S. (4′S.) | | | |
| | Em. * 4 . | | 20 29 | or 15$^{h}$ 15′ MT. (3′N.) |
| | Venus . . | | 4 17 | 20 35 N |
| | Mercury . | | 4 33 | 17 58 N |
| 10 | Sun . . . . | | 5 14 | 23 3 N |
| | Im. * 1 . . | 6 | 12 24 | or 7$^{h}$ 8′ MT. |
| | *'s R.A. 16$^{h}$ 36′ Decl 25° 12′ S. (2′N.) | | | |
| | Em. * 1 . | | 13 23 | or 8$^{h}$ 7′ MT. (6′N.) |
| | Im. * 2 . . | | 15 8 | or 9$^{h}$ 51′ MT. |
| | *'s R.A. 16$^{h}$ 41′ Decl. 25° 18′ S. (7′N.) | | | |
| | Im. * 3 . . | | 15 21 | or 10° 4′ MT. |
| | *'s R.A. 16$^{h}$ 41′ Decl. 25° 17′ S. (8′N.) | | | |
| | Em. * 2 . | | 16 17 | or 11$^{h}$ 0′ MT. (11′N.) |
| | Em. * 3 . | | 16 17 | or 11$^{h}$ 0′ MT. (11′N.) |
| | Im. * 4 . . | | 16 32 | or 11$^{h}$ 15′ MT. |
| | *'s R.A. 16$^{h}$ 44′ Decl. 25° 32′ S. (3′S.) | | | |
| | 25 Scorpii | 6 | 16 36 | 25 12 S |
| | 18 Oph. | 6 | 16 39 | 24 19 S |
| | Moon . . . | | 16 43 | 25 33 S |
| | 26 Oph. . | 6 | 16 49 | 24 43 S |
| | Em. * 4 . | | 17 30 | or 12 13 MT. (1′S.) |
| | Georgian . | | 19 4 | 23 3 S |
| | Venus . . | | 4 22 | 20 49 N |
| | Mercury . | | 4 32 | 17 47 N |
| 11 | Sun . . . . | | 5 18 | 23 7 N |
| | Moon . . . | | 17 40 | 25 31 S |
| | 63 Oph. . | 6.7 | 17 44 | 24 51 S |
| | 4 Sagit. . . | 5 | 17 49 | 23 47 S |
| | XVII. 342 | 7 | 17 54 | 24 24 S |
| | Georgian . | | 19 4 | 23 3 S |
| | Venus . . | | 4 27 | 21 3 N |
| | Mercury . | | 4 31 | 17 41 N |
| 12 | Sun . . . . | | 5 22 | 23 11 N |
| | Im. * . . . | 8 | 15 39 | or 10$^{h}$ 14′ MT. |
| | *'s R.A. 18$^{h}$ 32′ Decl. 24° 6′ S. (15′N.) | | | |
| | Em. * . . | | 16 8 | or 10$^{h}$ 43′ MT. (14′N.) |
| | XVIII. 129 | 6 | 18 28 | 23 39 S |
| | XVIII. 141 | 6 | 18 31 | 23 59 S |
| | Moon . . . | | 18 37 | 24 9 S |
| | ν Sagit. . | 5 | 18 44 | 22 57 S |
| | Georgian . | | 19 4 | 23 3 S |
| | Mercury . | | 4 30 | 17 36 N |
| | Venus . . | | 4 32 | 21 17 N |
| 13 | Sun . . . . | | 5 27 | 23 14 N |
| | Georgian . | | 19 4 | 23 4 S |
| | XIX. 138 . | 6 | 19 20 | 21 40 S |
| | XIX. 166 . | 7 | 19 25 | 21 9 S |
| | Moon . . . | | 19 30 | 21 37 S |
| | 56 Sagit. . | 6 | 19 36 | 20 10 S |
| | Mercury . | | 4 29 | 17 30 N |
| | Venus . . | | 4 37 | 21 32 N |
| 14 | Sun . . . . | | 5 31 | 23 18 N |
| | Im. * 1 . . | 7.8 | 14 52 | or 9$^{h}$ 20′ MT. |
| | *'s R.A. 20$^{h}$ 11′ Decl. 18° 52′ S. (9′S.) | | | |
| | Em. * 1 . | | 15 48 | or 10$^{h}$ 16′ MT. (2′N.) |
| | Im. * 2 . . | 8 | 15 53 | or 10$^{h}$ 21′ MT. |
| | *'s R.A. 20$^{h}$ 13′ Decl. 18° 54′ S. (0′) | | | |
| | Em. * 2 . | | 16 59 | or 11$^{h}$ 26′ MT. (7′S.) |
| | Im. * 3 . . | 5 | 19 0 | or 13$^{h}$ 27′ MT. |
| | *'s R.A. 20$^{h}$ 17′ Decl. 18° 46′ S. (cont.) | | | |
| | Georgian . | | 19 4 | 23 4 S |
| | Im. * 4 . . | 5 | 19 39 | or 14$^{h}$ 6′ MT. |
| | *'s R.A. 20$^{h}$ 19′ Decl. 18° 23′ S. (3′N.) | | | |
| | Im. * 5 . . | 7.8 | 19 43 | or 14$^{h}$ 10′ MT. |
| | *'s R.A. 20$^{h}$ 19′ Decl. 18° 27′ S. (1′S.) | | | |
| | XX. 45 . . | 8 | 20 6 | 16 49 S |
| | Moon . . . | | 20 21 | |
| | XX. 194 . | 7 | 20 26 | 17 7 S |
| | XX. 240 . | | 20 31 | 16 45 S |
| | Em. * 5 . | 6.7 | 20 53 | or 15$^{h}$ 20′ MT. (11′S.) |
| | Em. * 4 . | | 20 56 | or 15$^{h}$ 23′ MT. (8′S.) |
| | Mercury . | | 4 30 | 17 30 N |
| | Venus . . | | 4 42 | 21 43 N |
| 15 | Sun . . . . | | 5 35 | 23 20 N |

## JUNE.

| Days. | Planet's or Star's Name, &c. | Magnitude of Stars | Sidereal Time. | | Planet's or Star's Declination. | |
|---|---|---|---|---|---|---|
| | | | H. | M. | D. | M. |
| | Georgian . | | 19 | 4 | 23 | 4 S |
| | Im. * 7 . . | 7 | 19 | 51 | or $14^h$ 14′ MT. | |
| | *'s R.A. $21^h$ 8′ Decl. 14° 0′ S. (12′N.) | | | | | |
| | Em. * . . | | 21 | 1 | or $15^h$ 24′ MT. (0′) | |
| | Mercury . | | 4 | 31 | 17 | 30 N |
| | Venus . . | | 4 | 47 | 21 | 54 N |
| 16 | Sun . . . . | | 5 | 39 | 23 | 23 N |
| | Im. * 1 . . | 7.8 | 16 | 55 | or $11^h$ 14′ MT. | |
| | *'s R.A. $21^h$ 48′ Decl. 10° 24′ S. (11′S.) | | | | | |
| | Em. * 1 . | | 17 | 14 | or $11^h$ 33 MT. (15′S.) | |
| | Georgian . | | 19 | 3 | 23 | 4 S |
| | Im. * 2 . . | 7.8 | 20 | 44 | or $15^h$ 3′ MT. | |
| | *'s R.A. $21^h$ 54′ Decl. 9° 21′ S. (13′N.) | | | | | |
| | Em. * 2 . | | 21 | 48 | or $16^h$ 6′ MT. (1′N.) | |
| | Mercury . | | 4 | 31 | 17 | 30 N |
| | Venus . . | | 4 | 52 | 22 | 5 N |
| 17 | Sun . . . | | 5 | 43 | 23 | 24 N |
| | Georgian . | | 19 | 3 | 23 | 5 S |
| | Mercury . | | 4 | 32 | 17 | 36 N |
| | Venus . . | | 4 | 57 | 22 | 15 N |
| 18 | Sun . . . . | | 5 | 47 | 23 | 26 N |
| | Im. * . . . | 6 | 18 | 58 | or $13^h$ 9′ MT. | |
| | *'s R.A. $23^h$ 18′ Decl. 0° 10′ N. (7′S.) | | | | | |
| | Georgian . | | 19 | 3 | 23 | 5 S |
| | Em. * . . . | | 19 | 55 | or $14^h$ 6′ MT. (0′.) | |
| | Mercury . | | 4 | 32 | 17 | 41 N |
| | Venus . . | | 5 | 2 | 22 | 26 N |
| 19 | Sun . . . . | | 5 | 52 | 23 | 27 N |
| | Georgian . | | 19 | 3 | 23 | 5 S |
| | Mercury . | | 4 | 33 | 17 | 47 N |
| | Venus . . | | 5 | 8 | 22 | 37 N |
| 20 | Sun . . . . | | 5 | 56 | 23 | 28 N |
| | Georgian . | | 19 | 3 | 23 | 6 S |
| | Mercury . | | 4 | 35 | 17 | 57 N |
| | Venus . . | | 5 | 13 | 22 | 44 N |
| 21 | Sun . . . . | | 6 | 0 | 23 | 28 N |
| | Georgian . | | 19 | 3 | 23 | 6 S |
| | Mercury . | | 4 | 38 | 18 | 8 N |
| | Venus . . | | 5 | 19 | 22 | 51 N |
| 22 | Sun . . . . | | 6 | 4 | 23 | 28 N |
| | Georgian . | | 19 | 3 | 23 | 6 S |
| | Mercury . | | 4 | 40 | 18 | 18 N |
| | Venus . . | | 5 | 24 | 22 | 58 N |
| 23 | Sun . . . . | | 6 | 8 | 23 | 27 N |
| | Georgian . | | 19 | 3 | 23 | 7 S |
| | Im * . . . | 6.7 | 20 | 13 | or $14^h$ 5′ MT. | |
| | *'s R.A. $3^h$ 28′ Decl. 22° 5′ N. (13′S.) | | | | | |
| | Em. * . . . | | 20 | 47 | or $14^h$ 38′ MT. (7′S.) | |
| | Mercury . | | 4 | 43 | 18 | 32 N |
| | Venus . . | | 5 | 29 | 23 | 4 N |
| 24 | Sun . . . . | | 6 | 12 | 23 | 26 N |
| | Georgian . | | 19 | 2 | 23 | 7 S |
| | Mercury . | | 4 | 45 | 18 | 46 N |
| | Venus . | | 5 | 35 | 23 | 11 N |
| 25 | Sun . . . . | | 6 | 16 | 23 | 25 N |
| | Georgian . | | 19 | 2 | 23 | 7 S |
| | Mercury . | | 4 | 48 | 19 | 0 N |
| | Venus . . | | 5 | 40 | 23 | 18 N |
| 26 | Sun . . . . | | 6 | 21 | 23 | 23 N |
| | Georgian . | | 19 | 2 | 23 | 7 S |
| | Mercury . | | 4 | 52 | 19 | 16 N |
| | Venus . . | | 5 | 45 | 23 | 21 N |
| 27 | Sun . . . . | | 6 | 25 | 23 | 20 N |
| | Georgian . | | 19 | 2 | 23 | 8 S |
| | Mercury . | | 4 | 56 | 19 | 32 N |
| | Venus . . | | 5 | 51 | 23 | 25 N |
| 28 | Sun . . . . | | 6 | 29 | 23 | 18 N |
| | Georgian . | | 19 | 2 | 23 | 8 S |
| | Mercury . | | 5 | 0 | 19 | 48 N |
| | Venus . . | | 5 | 56 | 23 | 28 N |
| 29 | Sun . . . . | | 6 | 33 | 23 | 15 N |
| | Georgian . | | 19 | 1 | 23 | 8 S |
| | Mercury . | | 5 | 4 | 20 | 6 N |
| | Venus . . | | 6 | 1 | 23 | 32 N |
| 30 | Sun . . . . | | 6 | 37 | 23 | 11 N |
| | Georgian . | | 19 | 1 | 23 | 9 S |
| | Mercury . | | 5 | 9 | 20 | 24 N |
| | Venus. . . | | 6 | 7 | 23 | 36 N |

# ART. XI. ASTRONOMICAL AND NAUTICAL COLLECTIONS.

## No. XVII.

### i. *Remarks on the* CATALOGUE *of the Orbits of the* COMETS *that have been hitherto computed.* By DR. OLBERS.

THE Catalogue of the Orbits of Comets is founded on that which Delambre has given in the third volume of his Astronomy, p. 409. Many errors of the pen and of the press, in Delambre's Catalogue, are corrected, and those orbits are added which were unknown to Delambre, or overlooked by him, or which have been computed since the termination of his catalogue in 1813. Where several persons have computed the orbits of the same comets, some of their results have been omitted, when they have been manifestly incorrect, or derived only from a construction, or given merely as examples of computation with inadequate observations, and by no means intended to represent the correct orbits. Perhaps, however, too many inaccurate computations have still been retained: but this has been done with the intention of affording a conjecture how far the orbit may be more or less remote from a parabola: and where the orbit has been found decidedly elliptical, it is interesting to compare the difference of the parabolic and the elliptic elements. And since so many orbits have now been computed as elliptic or hyperbolic, a separate column has been added for the eccentricities. Where this is left blank, the eccentricity is supposed to be $= 1$, or the orbit to be parabolic. The eccentricity shows whether the orbit is elliptic or hyperbolic, and thus renders the elements complete, since the greater axis is easily found from the eccentricity and the least distance. The logarithm of the mean motion is assigned in all cases, on account of its utility in computing the true anomaly, even in the cases of elliptic and hyperbolic orbits. For this logarithm of the mean motion we have retained, on account of uniformity, the constant logarithm 9.9601283, which has hitherto been commonly used, as the logarithm of the mean motion of a comet, of which the least distance is $= 1$. This value supposes properly that the mass of the comet is equal to that of the earth: but if this mass, which is indeed unknown, but which is certainly always very inconsiderable became $= 0$, the logarithm should be 9.9601277: so that if we required the greatest possible accuracy, it would be necessary to diminish the tabular logarithm of the mean motion by 6 in the 7th place of decimals.

With respect to the following remarks on the table of comets, I must gratefully acknowledge the assistance that I have received from the excellent notes which the Baron von Zach and the Baron von Lindenau have respectively added to their tables. But for the sake of brevity, I have omitted many references which may be found in Pingré, or in other works here quoted, and very extensively circulated.

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1. 240. Chinese observations. A very uncertain orbit. Mon. Corr. X. p. 167.
2. 539. From Chinese observations, without any latitudes. Mém. prés. à l'Inst. I. p. 290. Mon. Corr. II. p. 415. XVI. p. 498.
3. 565. Deduced from two Chinese observations only, upon the two suppositions, that the curtate distance of the comet, at the time of the first observation, was either $= 1.2$ or $= 1.3$. Al-

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though the elements have some resemblance with those of the comets of 1683 and 1739, yet Burckhardt found that neither of these two orbits would accord with the observations of 565. Mon. Corr. X. p. 162.

4. 837. Chinese observations. Pingré Com. I. p. 340.

5. 989. Chinese observations. A very uncertain orbit. Mon. Corr. X. p. 167.

6. 1066. Very uncertain orbit. Pingré I. p. 373.

7. 1097. From Chinese observations of the 6th, 16th, and 17th October. Sav. Etr. I. p. 290. Mon. Corr. II. p. 417. XVI. p. 501.

8. 1231. Chinese observations. Pingré I. p. 401.

9. 1264. Phil. tr. XLVII. p. 281. Pingré I. p. 406. Mém. Par. 1760. p. 195. Struyck Vervolg Amst. 1753. p. 108, 109. The identity with the comet of 1556 is uncertain from the want of precision in both orbits.

10. 1299. Two European observations, and one Chinese: a third European record does not agree. Pingré I. p. 418.

11. 1301. Pingré has applied a correction to the European, and Burckhardt to the Chinese observations, which could not otherwise be reconciled. Hence the diversity. Pingré I. p. 420. Mon. Corr. X. p. 164.

12. 1337. Pingré I. p. 432. The orbit of Pingré is preferable to that of Halley, since it represents both the European and the Chinese observations tolerably well, while Halley's differs as far as 20° from the latter.

13. 1351. Even the few elements, which Burckhardt has been able to assign, are very uncertain. Pingré I. p. 437. Mon. Cor. II. p. 415. Mem. Sav. Etr. I. p. 290. There are only four Chinese observations, of the 24th, 26th, 29th, and 30th November, without latitudes. On the whole we can place no manner of reliance on the orbits of the comets of 240, 539, 565, 989, 1066, 1097, 1231, 1299, 1301, 1351, and 1362.

14. 1362. Mon. Corr. X. p. 166. Three Chinese observations. The two orbits are derived from different suppositions respecting the latitudes.

15. 1456. The celebrated comet of Halley, of which the period amounts to about 76 years. Pingré I. p. 459.

16. 1472. From the observations of Regiomontanus. Pingré I. p. 475.

(15.) 1531. Halley's comet as observed by Apian. Pingré I. p. 488. See also especially Halley's Tabulæ Astronomicæ, and his essay there inserted, De motu cometarum elliptico.

17. 1532. Pingré I. p. 492. The once supposed identity of this comet with that of 1661 must be abandoned. Méchain, Mém. Prés. X. p. 333. Olbers in Hindenburg's Magazine for Mathematics 1787. p. 440.

18. 1533. Pingré I. p. 496. The total diversity of the two orbits sufficiently shows the uncertainty of both. Struyck, 1753. p. 24. Astron. Jahrb. Berl. 1800.

(9.) 1556. From the observations of Paul Fabricius between the 4th and the 17th of March, which cannot be considered as certain; so that we can place little reliance on the resemblance to the still less certain elements of the comet of 1264. Pingré I. p. 502.

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19. 1558. From three observations of the Landgrave of Hesse, and one of Cornelius Gemma, the latter being corrected in what is, very probably, an error of the press. Gemma de Naturæ Divinis Characterismis. Book II. ch. i. p. 33. See Berl. Astr. Jahrb. 1817, p. 176.

20. 1577. From Tycho's Observations. Pingré I. p. 511.

21. 1580. Halley from Möstlin, Pingré from Tycho's better observations. Pingré I. p. 521.

22. 1582. Both orbits uncertain, since they are founded only on three observations of Tycho, of the 13th, 17th, and 18th of May; that of the 18th giving a double result, whence the two orbits are derived. The first elements seem the most probable, Pingré I. p. 544.

23. 1585. From the observations of Tycho and Rothmann. Pingré I. p. 550.

24. 1590. Tycho's observations, from 23 February to 6 March. Pingré I. p. 554.

25. 1593. According to the observations of Chr. J. Ripensis, at Zerbst. Mém. Par. 1747, p. 562. Pingré I. p. 557.

26. 1596. Halley from Möstlin, Pingré from Tycho's observations: hence the latter elements are preferable. Pingré I. p. 562.

(15) 1607. Halley's comet. Pingré II. p. 3. Halley's Tab. Astr. First supplement of the Berl. Astr. Jahrb. Mon. Corr. X. p. 425.

27. 1618. From Kepler's imperfect observations. Kepler de Cometis. Pingré II. p. 4.

28. 1618. Bessel's orbit is far the best, being founded on the observations of Harriot, Longomontanus, Cysat, and Schnellius. Berl. Astr. Jahrb. 1808, p. 113.

29. 1652. From the observations of Hevelius between the 20th December and the 8th January. Hevelius's observations are not only in the second volume of the Machina Cœlestis, which is very rare, but also in his Cometographia.

30. 1661. The observations of Hevelius from the 3d February to the 28th March. Machina Cœlestis II., and Cometographia. Mém. prés X. p. 350.

31. 1664. Hevelius's observations in the Prodromas Cometicus, or better in the Mantissa Prodromi, and in the Machina Cœl. II. p. 439. Pingré II. p. 10.

32. 1665. From Hevelius's observations from the 6th to the 20th April, which are found in the Descr. Comet. 1665, Mantissa Prodr. Com. and Mach. Cœlestis II.

33. 1672. According to Hevelius's observations from the 6th March to the 21st April. Mach. Cœl. II. p. 593.

34. 1677. According to Hevelius's observations from the 29th April to the 8th May. Flamstead observed it also twice. Mach. Cœl. II. p. 292. Flamstead Hist. Cœl. Br. Ed. 1712, p. 103. Ed. 1725. I. p. 103.

35. 1678. From Lahire's observations, which are only estimated, and from the chart in the Hist. Cél. of Lemonnier, p. 238. See particularly Struyck, 1753, p. 38, 39.

36. 1680. Euler's elliptical elements are to be considered merely as an example of calculation, and require no further consideration. It is only the elliptical orbit of Encke that is of any value at present; it is taken from his masterly prize essay on this comet

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(Zeitschrift für Astr. 1818), in which all the observations are collected and discussed. The first orbit of Encke is the parabola which agrees best with the observations. The longitudes are reckoned from the mean equinox of the 18th December, 1680.

(15.) 1682. Halley's comet: observed from 25th August to 19th September. Flamst. H. C. Br. I. p. 108. Hevel. Ann. Climact. p. 120. Halley in Tab. Astr. de mot. com. ellipt.

37. 1683. According to Flamstead's observations, from the 23d July to the 5th Sept. Flamst. p. 110.

38. 1684. According to Bianchini's observations, from the 1st to the 17th July. Phil. Tr. N. 169, p. 920. Acta erud. 1685. p.241.

39. 1686. Seen first in August in the East Indies, then in September, in Europe. Orbit not very certain. Pingré II. p. 28.

40. 1689. Very uncertain observations, Pingré II. p. 29.

41. 1695. Burckhardt computed his orbit from manuscript observations left by Delisle in the Dépôt de la Marine. What was before known of this comet Pingré has collected. p. 33. Conn. des tems, 1817, p. 278.

42. 1698. The observations of Lahire and Cassini, the only ones that we have of this comet, are deficient in accuracy. Anc. Mém. II. p. 341. X. p. 742. Mém. 1701, p. 117.

43. 1699. Observed by Fontenay at Pekin, and by Cassini and Maraldi at Paris. The observations extend from the 17th February to the 2d March. Mém. Par. 1701, p. 47.

44. 1701. From observations made by P. Pallu at Pau, which had lately been recovered, and from the observations of P. Thomas, at Pekin. Conn. des tems, 1811, p. 482. Noel Obs. Phys. Math. in India fact. p. 128.

45. 1702. The observations between the 20th April and the 5th May not very exact. Struyck, 1753, p. 50. Pingré II. p. 38. Mém. Inst. II. p. 28. Mon. Cor. XVI. p. 511.

46. 1706. Cassini and Maraldi, from the 18th March to the 16th April. Mém. Par. 1706, p. 91, 148. Pingré II. 39. Struyck, 1753, p. 54.

47. 1707. The observations extend from the 25th November to the 23d January, 1708. Mém. Par. 1707, p. 588, and 1708, p. 89, 323. On the orbits see Pingré II. p. 40. Struyck, 1753, p. 54. The orbit of Hottuyn, given imperfectly by Struyck himself, depends only on a construction.

48. 1718. From Kirch's observations, which are not particularly accurate. Misc. Ber. III. p. 200. Phil. Trans. XXX. XXXII. Pingré II. p. 41. Struyck Inleiding de Algemeene Geographie, p. 295. Struyck, 1753, p. 57.

49. 1723. Was seen in the East Indies as early as the 12th October. The orbits are principally founded on the observations made between the 20th October and the 18th December by Halley, Bradley, Pound, and Graham. Phil. Trans. XXXIII. n. 382, p. 41, n. 397, p. 223. The second orbit, ascribed to Struyck, is only found in the astronomical tables of Berlin. Pingré II. p. 42. Burckhardt's hyperbolic orbit. Conn. des tems, 1821.

50. 1729. Discovered by Father Sarabat the 31st July, 1729, and observed until the 18th Jan. 1730. Pingré II. p. 42. Struyck, 1740, p.

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297, 1753, p. 58. Mém. Par. 1730, p. 284. The hyperbolic and parabolic orbits of Burckhardt. Conn. des tems, 1821.

51. 1737. Computed from Bradley's own observations, extending from 26 Feb. to 2 April. Phil. Trans. N. 446, p. 111. Pingré II. p. 45. Struyck, 1740, p. 301.

52. 1737. The observations made at Pekin were published in the Mon. Corr. XXI. p. 316. Conn. des tems. 1812, p. 409.

53. 1739. The observations are by Zanotti, from the 28th May to the 18th August. N. Acta Erudit. 1740, p. 166. Comm. Inst. Bon. II. p. iii, p. 73. Struyck, 1753, p. 64. The second orbit, by Zanotti, is only a first approximation, still remaining imperfect.

54. 1742. For the numerous observations of this comet see Pingré II. 47.

55. 1743. In part very imperfectly observed. The observations are principally collected in Struyck, 1753, p. 75.

56. 1743. Observed imperfectly, and by Klinkenberg alone, from 18 Aug. to 13 Sept. Struyck, 1753, p. 76, 77. The observations, which are also inserted by Pingré II, p. 52, differ sometimes 1° and more from the elements assigned.

57. 1744. Besides the observers and computers of this celebrated comet quoted by Pingré, II. p. 52, and Struyck, p. 78, some valuable matter may be found in Chéseaux Traité de la Comète, Laus. 1744, and Hiorter Trans. Swed. Acad. of Sciences.

58. 1747. Discovered by Chéseaux the 13th Aug. 1746, and last observed by Maraldi the 5th Dec. 1746. The orbits of Maraldi and Lacaille are the best. Pingré II. p. 57. Struyck, 1753. p. 92.

59. 1748. Especially observed by Maraldi, Mém. Par. 1748, p. 229.

60. 1748. Observed only three times imperfectly by Klinkenberg, the 19th, 20th, and 22d May. Struyck, 1753, p. 96. Bessel has reduced the observations with greater care. Berl. Astr. Jahrb. 1809, p. 99. The imperfect elements, time of the Perihelium, 1748, 22d April, ☊ $9^{s}$, 24°, Inclination 76°, Least distance 0.5, Motion retrograde, which Delambre ascribes to Burckhardt, and respecting which I can find no further information, cannot possibly belong to either of the computed comets of this year: so that they must have been derived from the alleged observations of the silly Kinderman, which deserve no credit whatever.

61. 1757. Bradley's observations and elements are preferable to the rest. Phil. tr. L. Part. i. p. 408. The other observations may be found collected by Pingré, Mém. Par. 1757, p. 97.

62. 1758. Messier observed the comet from the 15th Aug. to the 2d Nov. Mém. Par. 1759, p. 154, 1760, p. 165, 463.

(15). 1759. Celebrated and predicted re-appearance of Halley's comet. Pingré II. p. 63, gives references to works in which the observations and the elements may be found. Burckhardt's orbit, preferable to all the rest. Conn. des tems, 1819.

63. 1759. Pingré prefers his own elements. The comet was observed from the 25th June to the 16th March 1760. Pingré II. p. 68.

64. 1759. Observed from the 8th Jan. to the 8th Feb. 1760. Pingré II. p. 70.

65. 1762. Discovered by Klinkenberg the 17th May, observed to the 2d July. The refraction had been neglected in the reduction of

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the observations, and hence all the computed orbits varied several minutes from the observations. Burckhardt reduced them with greater care, and hence obtained the last elements, which are more correct. Mém. Inst. VII. p. 226. Mon. Corr. XVI. p. 515.

66. 1763. Discovered by Messier the 28th Sept., observed before the perihelium from the 30th Sept. to the 25th Oct., after the perihelium from the 12th to the 25th Nov. Pingré and Lexell could not represent the observations sufficiently well by any conic section. Pingré II. p. 106. Acta. Ac. Sc. Petr. 1780. Pt. ii. p. 324. Burckhardt has corrected the observations of Messier, which were distorted by some errors in the places of Flamstead's stars, and he has employed the observations of St. Jaques de Silvabelle, which were made public more lately. Mon. corr. X. p. 507. Conn. des tems. XIII. p. 344.

67. 1764. Discovered by Messier, and observed from 3d Jan. to 11th Feb. The third orbit is that which has been corrected from all the observations. Pingré II. p. 74.

68. 1766. Discovered by Messier the 8th March, and observed for eight days only. Pingré II. p. 75.

69. 1766. Observed by Messier at Paris only five days, from the 8th to the 12th April. La Nux at the Isle of Bourbon followed it from the 29th April to the 13th May. Pingré II. p. 76. The imperfect observations of La Nux, Pingré could not satisfactorily combine with the Parisian observations, and Burckhardt has attempted to do this by means of an ellipsis. Conn. des tems. 1821.

70. 1769. Discovered by Messier the 8th Aug., and observed before the perihelium to the 15th Sept.; after it from the 24th Oct. to the 1st Dec. The principal observations are found Mém. Par. 1769, p. 49; 1770, p. 24; 1775, p. 392. Maskelyne Astr. Obs. I. On the various orbits, besides Pingré II. p. 83, see especially Euler Recherches et Calculs sur la comète, 1769; 4 Pet. 1770; the two rare works of Asclepi De cometarum motu exercitatio; 4 Rom. 1770, and De cometarum motu Addenda, Rom. 1770; besides Bessel's excellent prize memoir in the Berl. Astr. Jahrb. 1811. That Bessel's elliptic orbit is preferable to all others, scarcely requires to be observed. The nodes and perihelium are determined by Bessel as at rest with respect to the stars for the 1st Jan. 1769.

71. 1770. This comet, celebrated for an orbit deviating so greatly from the parabolic form, was discovered the 14th June by Messier, and observed till the 2d Oct. The observations have been collected by Messier, Mém. Par. 1776, p. 597. The short period of this comet, little exceeding five years and a half, which appeared so paradoxical when it was first computed by Lexell, was fully confirmed by Burckhardt's investigations in his valuable prize memoir. Mém. Inst. 1806, p. 1. The reason why the comet has not re-appeared since 1770, has been very satisfactorily explained in Laplace's Méc. Cél. vol. IV.

72. 1770. Was observed only four times at Paris, between the 10th and 20th of Jan. 1771. Mém. Par. 1771. Pingré II. p. 90.

73. 1771. Discovered by Messier the 1st April, and observed by him until the 19th June; but by St. Jaques de Silvabelle at Mar-

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seilles till the 17th July. The orbit appears, according to the investigations of Burckhardt and of Encke, to be truly hyperbolical. The parabola of Encke, however, affords also results varying but little from the truth. Mon. Corr. X. p. 510. Conn. des tems XIII. p. 344. Von Zach Corresp. astr. 1820. Encke reckons from the mean equinox, 1st Jan. 1771.

74. 1772. Discovered by Montaigne the 8th March, observed by Messier four times only, the 26th, 27th, and 30th March, and the 1st April. The computation of the elliptic orbits was undertaken on account of the similarity of the elements of the second comet of 1805. These more correct investigations render the identity of the two comets highly improbable, and it is accordingly rejected by Bessel, as well as by Burckhardt, who was able to employ the rediscovered observations of Montaigne. Mon. Corr. XIV. p. 73, 84. Conn. des tems 1811, p. 486.

75. 1773. Discovered by Messier the 12th Oct. 1773, and observed until the 14th Apr. 1774. Burckhardt, who employed also observations of St. Jaques de Silvabelle, found the orbit not sensibly different from a parabola. Mém. Par. 1774—1777. Acta Petr. 1779, p. 335. Conn. des tems XIII. p. 343. Mon. Corr. X. p. 540.

76. 1774. Discovered by Montaigne the 11th Aug. observed till the 25th Oct. Mém. Par. 1775, p. 445. Conn. des tems 1821.

77. 1779. Discovered by Bode the 6th Jan. observed till the 17th May. Mém. Par. 1779, p. 318. Pingré, II. p. 94, seems to prefer the orbit of Dangos before the rest.

78. 1780. Discovered by Messier the 27th Oct. and observed till the 29th Nov. Mém. Par. 1780, p. 515. Act. Petr. 1780, p. ii. p. 347. Berl. Astr. Jahrb, 1784, p. 141.

79. 1780. Discovered the 18th Oct. by Montaigne and Olbers, and observed very imperfectly three times only. The elements are therefore very uncertain. Mém. Par. 1780, p. 515. Berl. Astr. Jahrb. 1804, p. 172.

80. 1781. Discovered by Méchain the 28th June, observed till the 15th July. Mém. Par. 1781, p. 349; 1782, p. 581. Berl. Astr. Jahrb. 1785, p. 166.

81. 1781. Discovered by Méchain the 9th Oct. observed till the 25th Dec. Mém. Par. 1781, p. 360; 1782, p. 587. Legendre Nouv. Méthode, p. 41.

82. 1783. Discovered by Pigott the 20th Nov. observed till the 21st Dec. Mém. Par. 1783, p. 123, 643, Phil. tr. LXXIV. Conn. des tems 1788. But respecting Burckhardt's orbit, see especially Conn. des tems 1820, p. 305.

83. 1784. Seen by De la Nux at the Isle of Bourbon the 15th Dec. 1783, in Paris not till the 24th January, and observed there till the 11th March. Afterwards it was again visible, and observed from the 9th to the 26th of May. Mém. Par. 1784, p. 313, 358. The first elements are the most correct.

The comet hitherto inserted in the tables, as the second of 1784, is a shameful forgery of the Chevalier Dangos, as Professor Encke has shown in the Corresp. Ast. for 1820. Cah. v. p. 456.

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84. 1785. Discovered by Messier and Méchain the 7th Jan. observed till the 8th Feb. Mém. Par. 1785, p. 646. Berl. Astr. Jahrb. 1789, p. 142. Conn. des tems, 1788.

85. 1785. Discovered by Méchain the 11th March, observed till the 17th Apr. Mém. Par. 1785, p. 646. Berl. Astr. Jahrb. 1789, p. 143.

86. 1786. Was discovered by Méchain the 17th Jan. and could only be observed once more on the 19th, by Méchain and Messier. Mém. Par. 1786, p. 95. But since the identity of this comet with those of the years 1795, 1805, and 1819, has been demonstrated, Encke was able to determine the orbit from these two observations. Berl. Astr. Jahrb. 1822. Corresp. Astr. 1819. Conn. des tems, 1819, p. 224.

87. 1786. Discovered the 1st Aug. by Miss Caroline Herschel, observed till the 26th Oct. Mém. Par. 1786, p. 98. Maskelyne, Astr. Obs. II. p. 29. Ephem. Milan. 1789. Conn. des tems, 1789.

88. 1787. Discovered by Méchain the 10th April, observed at Paris till the 20th; at Marseilles till the 26th May. Mém. Par. 1787, p. 70. Conn. des tems, 1790. Berl. Astr. Jahrb. 1791. La Nux observed it at the Isle of France from the 25th May to the 21st June.

89. 1788. Discovered by Messier the 25th Nov. observed till the 29th Dec. Mém. Par. 1789, p. 663. Conn. des tems. 1791. Berl. Astr. Jahrb. 1793.

90. 1788. Discovered by Miss Herschel the 21st Dec.: observed last by Méchain the 18th Jan. 1789. Phil. tr. LXXVII. p. 1. Mém. Par. 1789, p. 681. Maskelyne Astron. Obs. for 1788. Berl. Astr. Jahrb. 1793. p. 119.

91. 1790. Discovered by Miss Herschel the 7th Jan.: observed only four times; the 9th, 19th, 20th, and 21st Jan. Mém. Par. 1790, p. 309. The first orbit agrees best with the longitudes; the second with the latitudes.

92. 1790. Discovered by Méchain the 9th of Jan. and observed till the 22d. Mém. Par. 1790, p. 313. Conn. des tems, 1792, p. 355. Berl. Astr. Jahrb. 1794.

93. 1790. Discovered by Miss Herschel the 17th April, and observed till the 29th June. Mém. Par. 1790, p. 320. Conn. des tems 1792, p. 355. Englefield on Comets, Lond. 1793, p. viii.

94. 1792. Discovered by Miss Herschel the 15th of December 1791: observed last by Maskelyne on the 25th Jan. 1792. Berl. Astr. Jahrb. 1795, p. 184, 201; 1796, p. 148; Conn. des tems 1793, p. 374. Englefield on Comets. The first elements, by Méchain, are those which have been corrected from all the observations.

95. 1792. Discovered by Méchain the 10th Jan. 1793; also by Piazzi; observed till the 19th Feb. Piazzi della spec. astr. book v. Berl. Astr. Jahrb. 1797, p. 136; 1799, p. 192. Conn. des tems, 1795, p. 286.

96. 1793. Discovered by Messier the 27th Sept. observed till the 11th Oct.: then seen again the 30th Dec. and observed till the 4th Jan. 1794. Conn. des tems, 1795, p. 287.

(To the best of my knowledge, Messier's observations of this comet, as well as of some others, have not yet been printed. Their entire publication would be highly desirable.)

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97. 1793. Discovered by Perny the 24th September, observed till the 3d December. Conn. des tems, 1795, and especially Conn. des tems, 1820. Burckhardt leaves it doubtful whether this comet is or is not identical with that of 1783.

(86.) 1795. Discovered by Miss Herschel, the 7th November, and observed till the 27th. The observations chiefly rather inaccurate. This is the second appearance of Encke's comet. Phil. Trans. 1795. Berl. Astr. Jahrb. 1799. 1814. More especially see Berl. Astr. Jahrb, 1822, p. 183. Von Zach Corr. Astr. 1819. Encke computes from the mean equinox of the 18th November, 1795.

98. 1796. Discovered by Olbers the 31st March, and observed till the 14th April. Berl. Astr. Jahrb. 1799, p. 106.

99. 1797. Discovered by Bouvard the 14th August, and observed till the 31st. Berl. Astr. Jahrb. 1800. Allg. Geogr. Ephem. I. p. 127, 365, 604.

100. 1798. Discovered by Messier, the 12th April, and observed till the 24th May. Mém. Inst. II. p. 430. Allg. G. Eph. I. p. 679, 692, 694. II. p. 79, 95. Berl. Astr. Jahrb. 1801, p. 231.

101. 1798. Discovered the 6th December by Bouvard, and the 8th by Olbers, and only observed till the 12th. Berl. Astr. Jahrb, 1802, p. 195. Allg. G. Eph. III. Conn. des tems, 1804, p. 373.

102. 1799. Discovered the 6th of August, by Méchain, and observed till the 21st October. Allg. G. Eph. IV. Berl. Astr. Jahrb. 1802, 1803. Mon. Corr. I. II.

103. 1799. Discovered the 26th Dec. by Méchain, and observed by him till the 5th Jan. 1800. Mon. Corr. I. p. 191, 299, Mém. Inst. II. p. 153. Berl. Astr. Jahrb. 1803. Conn. des tems, 1804, p. 376. Méchain thinks that this comet may possibly have been identical with that of 1699.

104. 1801. Discovered almost the same hour by Pons, at Marseilles, and Messier, Méchain, and Bouvard, at Paris, on the 12th July: observed last by Méchain, the 23d. Mon. Corr. IV. p. 179. Berl. Astr. Jahob. 1805, p. 129. Conn. des tems, An. XIII. p. 344, 484.

105. 1802. Discovered by Pons the 26th August: observed till the 3d October. Mon. Corr. VI. Conn. des tems. An XIII. p. 236, 374. Berl. Astr. Jahrb. 1805, 1806, p. 129.

106. 1804. Discovered by Pons the 7th March: observed till the 1st April. Conn. des tems, XV. p. 374: 1808, p. 335. Mon. Corr. IX. Berl. Astr. Jahrb. 1807.

(86.) 1805. Third appearance of the comet of Encke. Discovered at the same time by Bouvard, Pons, and Huth, the 20th October: observed till the 15th, and seen on the 19th November. Mon. Corr. XIII. XIV. Conn. des tems, 1808, 1809. Berl. Astr. Jahrb. 1809. But especially see Berl. Astr. Jahrb. 1822, 1823. Corresp. Astr. 1819.

107. 1805. Discovered by Pons the 10th Nov. and observed till the 9th Dec. Its supposed identity with the comet of 1772 has given occasion to many computations. This identity has not been confirmed; but Gauss found that the observations agree better with any ellipsis that has its greater semiaxis longer than 2.82, than with a parabola. Mon. Corr. XIII. XIV. XXVI.

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p. 360, 490. Berl. Astr. Jahrb. 1809. Conn. des tems. 1808, 1809.

108. 1806. Discovered by Pons the 10th November: observed first till the 20th Dec., and then again from the 17th Jan. to the 12th Feb. 1807. Mon. Corr. XV. XVI. Berl. Astr. Jahrb. 1810. Conn. des tems, 1810. p. 298, 1819.

109. 1807. Great comet. Observed from the 22d Sept. 1807 to the 27th March, 1808. Most of the observations are collected in Bessel's Untersuchungen über den grossen cometen, 4, Königsberg, 1810. Besides this classical work see also Mon. Corr. XVI. XVII. XVIII. XIX. Berl. Astr. Jahrb. 1811, 1812, 1813, 1814. Cacciatore della cometa di 1807. Conn. des Tems, 1809, 1810, 1811, and Phil. Trans. 1808.

110. 1808. Discovered by Pons, the 24th June, and somewhat uncertain, especially with respect to the declinations: observed only at Marseilles, from the 26th June till the 3d July. Mon. Corr. XVIII. p. 245, 359. Berl. Astr. Jahob. 1812, p. 129.

111. 1810. Discovered the 22d August by Pons, and observed very doubtfully at Marseilles only from the 29th Aug. to the 21st Sept. Mon. Corr. XXIII. p. 302., XXIV. p. 71. Berl. Astr. Jahrb. 1814, p. 179.

112. 1811. Discovered by Flauguergues the 26th March, and observed in Europe before the perihelium till the 2d June, after the perihelium from the 20th August to the 11th Jan. 1812: lastly rediscovered by Wisniewski, at New Tsherkask, the 31st July, 1812, and observed again from the 8th to the 17th of August. Upon this great comet, very remarkable even in its form, and observed by almost all astronomers, besides the Mon. Corr. XXIII. XXIV. XXV. Phil. Trans. 1812. Berl. Astr. Jahrb. 1815, 1816. Conn. des tems, 1820, and so forth, see especially the excellent treatise of Dr. F. W. A. Argelander, Ueber die Bahn des grossen cometen von 1811, 4, Königsberg, 1822. The orbit of Argelander, inserted in the table, is that which he considers as the most probable: but the observations made at the different times of the comet's appearance could not be quite satisfactorily represented by any orbit governed by the Keplerian laws. Argelander reckons from the place of the mean equinox the 12th Sept. 1811.

113. 1811. Discovered by Pons the 16th November, and observed last at Bremen, the 16th February, 1812. Conn. des tems, 1820. Mon. Corr. XXIV, XXV, and especially XXVII. Nicolai reckons from the mean equinox on the 1st Jan. 1812.

114. 1812. Discovered by Pons the 20th July, and observed till the end of September. Mon. Corr. XXVII. XXVIII. Con. des tems, 1820: but see especially the Zeitschrift for 1817. Encke reckons the longitudes from the mean equinox of the 1st September, 1812.

115. 1813. Discovered by Pons the 4th February, and observed till the 11th March. Mon. Corr. XXVII. Conn. des tems, 1820.

116. 1813. Discovered by Pons at Marseilles, and Harding, at Gottingen, the 2d and 3d of April: observed till the 29th. Conn. des tems, 1820. Mon. Corr. XXVII. XXVIII.

117. 1815. Discovered by Olbers the 6th of March: observed last by Gauss, the 25th of August. Bessel has taken the perturbations into

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account for his elliptical orbit. Period, according to Nicolai, 74.7893 years; according to Bessel, 74.04913. Bessel computes, that taking all the perturbations into account, the comet will reach its perihelium again in as early as 1887, Feb. 9.4, that is 824.5 days earlier than the period of the simple elliptic orbit. Berl. Astr. Jahrb. 1818, 1819. Observations at Königsberg, II. Zeitschrift for 1816. Trans. Berl. Acad. 1812, 1815. Math. Class. Conn. des tems, 1820. Bessel reckons the longitudes from the mean equinox of the 1st Jan. Nicolai from the 26th April, 1815.

118. 1818. Discovered by Pons the 26th Dec. 1817: observed last at Bremen the 1st May 1818. The comet was on account of the faintness of its light very difficult to be observed, and appeared to be gradually dissolved. Berl. Astr. Jahrb. 1821. Zeitschrift for 1818. Conn. des tems, 1821.

119. 1818. Discovered by Pons the 29th Nov. 1818; afterwards by Bessel the 22d Dec. Last observed by Harding the 30th Jan. 1819. Berl. Astr. Jahrb. 1821, 1824. Conn. des tems, 1821. Corresp. Astr. II. p. 187.

(86.) 1819. Reappearance of the celebrated comet of Encke, by which its short period of 1207 days was first ascertained. Discovered by Pons the 27th Nov. 1818: observed last the 12th Jan. 1819. Only the last elliptic orbit of Encke is to be considered as the true one. Corr. Astr. 1819. Berl. Astr. Jahrb. 1822, 1823. Conn. des tems, 1822. Encke reckons the longitudes from the mean equinox of the 1st Jan. 1819.

120. 1819. Appeared suddenly in Europe emerging from the sun's rays in the beginning of July, of a considerable magnitude. Last observed in October at Dorpat and at Bremen. Is remarkable, because, according to the elements, it must have passed over the sun's disc on the 26th of June. Corr. Astr. 1819. Berl. Astr. Jahrb. 1821, 1822. Conn. des tems, 1822.

121. 1819. Discovered by Pons the 12th June, and only observed at Marseilles and in Milan till the 19th July. Only the last orbit, by Encke, agrees with the observations, which cannot be represented by any parabola. Corr. Astr. 1819. Berl. Astr. Jahrb. 1822, p. 243; 1823, p. 221. Efem. Milan. 1820. Encke computes from the mean equinox of the 1st Jan. 1819.

122. 1819. Discovered by Blanpain at Marseilles the 28th Nov.: observed last at Milan the 25th Jan. 1820. Observed also at Bologna, and especially at Paris. The great deviation of the orbit from a parabola is not to be doubted; but the limits of the time of revolution have not hitherto been found assignable, on account of the too short interval between the observations which have been published, and which are in some degree of doubtful accuracy; those of Marseilles not having been obtained by the most earnest entreaties and demands. Corresp. Astr. 1820. Berl. Astr. Jahrb. 1824. Conn. des tems, 1824. Encke reckons from the mean equinox of the 1st June, 1820.

123 1821. Discovered at the same time on the 21st Jan. by Nicollet at Paris, and by Pons at La Marlia. Was observed in Europe till the 7th March, and after the perihelium, from the 1st April till the 3d May at Valparaiso, by Captain Basil Hall, Lieutenant W. Robertson, and Mr. H. Forster. Its apparent

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motion was very slow throughout the time of the European observations. Brinkley's first elements are founded only on the observations at Valparaiso: the second he has computed from the observation of the 30th January at Bremen, and those of Capt. Hall, made the 8th April and the 3d May. Rosenberger was able to represent both the European and the American observations sufficiently well by his parabola. The rest of the orbits are founded on the European observations alone. The orbit seems to differ very little from a true parabola. Corr. Astr. 1820. Conn. des tems, 1824. Berl. Astr. Jahrb. 1824. Phil. Tr. 1822, Pt. i. Edinb. Phil. Journ. N. xiv. p. 382. Schumacher Astr. Nachr. N. 2, p. 17. N. 11, p. 166. N. 24, p. 425.

124. 1822. Discovered by Gambart at Marseilles the 12th May, by Pons at Marlia the 14th, and by the Oberlieutenant Biela, at Prague, the 16th: observed till the end of June. Zach Corresp. Astr. 1822, cah. iii. iv. v. Schumacher Astr. Nachr. N. 19, p. 298. N. 20, p. 309. Berl. Astr. Jahrb. 1825.

(86.) 1822. Orbit corrected by Encke from Rümker's observations at Paramatta. Schumacher Astr. Nachr. N. 27, p. 39. The longitudes relate to the mean equinox of the 24th May.

125. 1822. Discovered the 13th July by Pons, at Marlia: observed till the 22d October. Only the second ellipsis of Encke is founded on the whole apparent arc described: but the second parabolic orbit of Nicolai, and the third of Hansen represent also the whole of the observations hitherto published in the most satisfactory manner. Perhaps we may expect to receive from the Cape of Good Hope, or from New Holland, some observations of the two last comets, as well as of that which was discovered on the 31st May, by Pons, at Marlia, near β Piscium, and which was little observed in Europe, and has not yet been computed. Zach. Corr. Astr. cah. v. Schumacher Astr. Nachr. N. 20, p. 307.; n. 21. Suppl.; N. 22, p. 361, and 1. Suppl.; N. 23, p. 393, and Suppl.; N. 24, Suppl. Nicolai supposes the mean equinox of the 23d Oct.; Hansen in the second orbit, the 1st Sept.; in the third the 1st October, and Encke, the 25th Oct. 1822. [See also Gambart in Conn. des tems, 1826. Tr.]

## ii. *Further Remarks on the periodical* COMET (86 Olb.) *with conjectures on the effect of a resisting medium.*

By Professor ENCKE. *Bode's Alm.* 1826, p. 124.

The observations of Mr. Rümker have removed every possible doubt respecting the identity of the comet, and made it certain that future Astronomers will be able to recover it even if it should remain invisible for several revolutions. Fortunately, however, there is no reason to apprehend that it will escape us in its next visit. Dr. Olbers first pointed out to me that if it passed the perihelium later than the 10th of September, it will be visible to Europeans in August. From a cursory computation of the perturbations, I find that its perihelium will be about Sept. 16.4, and its places will be nearly these:

| | A. R. | | Decl. | | Log. Dist. | |
|---|---|---|---|---|---|---|
| 1825 | ° | ′ | ° | ′ | ☉ | ⊖ |
| Aug. 1.6 | 82 | 31 | 32 | 1 N | 0.023 | 0.162 |
| 6.6 | 90 | 23 | 32 | 9 | 9.988 | 0.141 |
| 11.6 | 99 | 1 | 31 | 44 | 9.948 | 0.123 |
| 16.6 | 108 | 19 | 30 | 36 | 9.903 | 0.107 |
| 21.6 | 118 | 9 | 28 | 37 | 9.852 | 0.097 |
| 26.6 | 128 | 14 | 25 | 40 | 9.792 | 0.092 |
| 31.6 | 138 | 23 | 21 | 46 | 9.724 | 0.093 |

Its next return to the perihelium will be in Dec. 1828, or perhaps in the beginning of January 1829: and it will then be easily visible, unless its light should prove to be gradually diminishing.

In attempting to compute the perturbations, it becomes necessary to employ higher powers of the quantities concerned than those which are sufficient for the planets, and no precautions or suppositions respecting the masses of the disturbing bodies are capable of representing the successive revolutions from each other without errors of several degrees. For example, we represent the five perihelia of 1786, 1795, 1805, 1819, and 1822, most conveniently by taking the mass of Jupiter $\frac{1}{12}$ less than that which is assigned by Laplace: but errors exceeding a whole day in the interval will still remain: and the middle three of the five considered alone afford still greater irregularities, although the actions of the planets which are neglected would have a very inconsiderable influence on this combination.

Comprehending the effects of ☿, ♀, and ⊖, the best result is obtained by increasing the mass of Jupiter about $\frac{1}{7}$: but the elements thus corrected give the perihelium of 1786 two days too early, and that of 1822 a day and a half too late: nor will any other planetary perturbation, that can be imagined, enable us to remove this error.

I therefore think myself entitled to consider it as demonstrated, that an alteration of the supposed masses of the planets alone is not sufficient for the purpose; and in the predictive computation of the places for 1822, which has now been verified, I was induced by this conviction to assume an empirical correction proportional to the square of the time.

Such a correction of the epoch corresponds to a mean angular motion increasing directly as the time, and points at once to the possibility of an explanation of its foundation. From Newton down to Laplace, a number of the most acute mathematicians have investigated the influence which any substance, scattered through space, might have on the motions of the heavenly bodies. The resistance of such a medium would necessarily occasion a continual shortening of the greater axis, and consequently an increase of the mean motion, and a diminution of the eccentricity; while the longitude of the perihelium would undergo only periodical changes compensating each other in the time of a revolution; and the nodes and inclination, which affect only the plane of the orbit, would be perfectly exempted from its influence. Now these are exactly the changes which are observable in our comet; for the decided change of the eccentricity, in the year 1822, may at least, in great measure, be attributed to some extraneous cause: though the earlier observations would not agree so well with a rapid change of this element.

Before it had been shown by these computations that the various returns of the comet could not be reconciled to each other without some such hypothesis, Dr. Olbers had already pointed out the probability of a similar alteration in a letter to Mr. von Lindenau. He wrote again to me on the subject in these words: "The exemption of the dense and solid bodies of the planets from any sensible effects of resistance, in the interplanetary spaces, proves nothing with respect to comets, which occupying, perhaps, a volume 1000 times as great, may have masses 1000 times as small: and with respect to this comet of 'Pons,' such a resistance seems to be almost demonstrable *à priori*: for it moves, during a considerable portion of its period, in that part of the open space of the system, in which the visible substance of the zodiacal light, or solar atmosphere, is found. It is this same comet, through the middle of which Herschel, on the 9th Nov. 1795, saw a small double star of the twelfth or thirteenth magnitude, with very little diminution of its brightness. This fact seems to demonstrate that the density of the comet bears some finite proportion to that of the zodiacal light, and that the substance causing this light may afford some sensible resistance to the motion of the comet. If then all the rest of the space surrounding us were to be considered as perfectly void and free from resistance, which I do not believe, still the substance of the zodiacal light, which does certainly exist, is sufficient to explain the phenomena of a diminution of the periodical time, and of the eccentricity of the orbit."

Dr. Olbers further remarks that this ethereal medium may naturally be supposed to revolve with the planets from west to east, so as to exhibit little or no influence on their motions, while those of the comets, being more discordant, may suffer a very material disturbance from its resistance.

[Upon a probable supposition respecting the law of the density of the resisting medium, Professor Encke proceeds to calculate the places of the comet for 1795, 1805, and 1819, in such a manner as to reduce the sum of the errors to less than half their amount upon the most advantageous suppositions respecting the planetary perturbations. For 1822 the improvement does not appear to be so considerable.]

The times of the successive passages through the perihelium, as affected by the perturbation, are assigned in this table, and those, which have been observed, are distinguished by an asterisc.

*1786 Jan. 30.1 Parisian M. T.
1789 May 18.7
1792 Sept. 4.1
*1795 Dec. 21.4
1799 April 11.1
1802 Aug. 1.9

*1805 Nov. 21.5
1809 March 11.9
1812 June 26.3
1815 Oct. 12.7
*1819 Jan. 27.3
*1822 May 24.0

The comet of 1766, which has been suspected to be the same, is recorded to have been very bright to the naked eye, and its motion was retrograde.

### iii. *Comparison of the* NEW TABLES *of Refraction with Observation.*

REFRACTIONS *observed by* MR. GROOMBRIDGE. (Coll. XIII. i.)

| Obs. | App. Alt. | Fahr. | M.Corr. for 1° | Refr. B.30 | Red. to 46°.5 | N. A. | Error. | Ivory 50°. | " Error" |
|---|---|---|---|---|---|---|---|---|---|
| | ° ′ ″ | ° | ″ | ′ ″ | ′ ″ | ′ ″ | ″ | ′ ″ | ″ |
| 16 | 1 18 42.6 | 49.2 | 3.5 | 22 23.3 | 22 32.7 | 22 16.2 | −16.5 | 22 10.2 | −10.8 |
| 25 | 1 30 9.5 | 38.1 | 3.3 | 21 38.0 | 21 10.2 | 21 6.1 | − 4.1 | 20 58.7 | + 7.2 |
| 17 | 2 0 8.1 | 52.9 | 2.7 | 18 11.1 | 18 28.3 | 18 28.5 | + 0.2 | 18 19.0 | − 0.4 |
| [44] | 2 17 35.2 | 31.9 | 2.4 | 17 38.9 | 17 4.0 | 17 10.7 | + 6.7 | 17 1.5 | + 2.6 |
| 13 | 2 30 53.4 | 56.7 | 2.3 | 15 51.6 | 16 15.0 | 16 18.6 | + 3.6 | 16 8.5 | − 2.3 |
| 9 | 2 41 2.2 | 56.3 | 2.1 | 15 18.3 | 15 38.8 | 15 39.3 | + 0.5 | 15 29.9 | − 5.1 |
| 5 | 2 51 34.5 | 28.7 | 2.1 | 15 32.7 | 14 55.3 | 15 2.6 | + 7.3 | 14 53.3 | +14.2 |
| 12 | 3 2 29.9 | 38.9 | 2.0 | 14 44.7 | 14 29.5 | 14 27.0 | − 2.5 | 14 18.0 | + 2.7 |
| 10 | 3 6 50.9 | 54.8 | 2.0 | 13 53.5 | 14 10.1 | 14 13.3 | + 3.2 | 14 5.5 | + 1.2 |
| 8 | 3 53 37.5 | 39.8 | 1.6 | 12 17.4 | 12 6.7 | 12 6.2 | − 0.5 | 12 0.8 | + 2.6 |
| 10 | 4 7 2.5 | 32.8 | 1.56 | 12 1.6 | 11 40.2 | 11 36.2 | − 4.0 | 11 31.9 | + 3.5 |
| 15 | 4 49 8.7 | 38.6 | 1.33 | 10 21.7 | 10 11.2 | 10 16.4 | + 5.2 | 10 11.8 | + 3.8 |
| 12 | 4 55 27.2 | 43.5 | 1.30 | 10 10 0 | 10 5.4 | 10 5.2 | − 0.2 | 10 1.0 | + 1.1 |
| 16 | 4 55 55.7 | 38.3 | 1.30 | 10 12.8 | 10 2.1 | 10 4.4 | + 2.3 | 10 0.2 | + 5.1 |
| | | | | | | | ±56.8 | | ±62.6 |
| | | | | | | | + 1.2 | | +25.4 |
| | | | | | | | | | Diff. |
| 7 | 5 9 17.7 | 58.2 | 1.25 | 9 29.8 | 9 44.4 | 9 43.0 | + 1.4 | 9 39.2 | + 0.6 |
| 11 | 5 27 58.5 | 33.8 | 1.20 | 9 28.5 | 9 13.3 | 9 13.8 | + 0.5 | 9 11.7 | + 2.1 |
| 13 | 5 44 33.2 | 53.8 | 1.13 | 8 41.4 | 8 49.7 | 8 53.0 | + 3.3 | 8 49.3 | + 0.3 |
| 13 | 5 47 55.9 | 33.6 | 1.12 | 8 57.5 | 8 43.1 | 8 47.6 | + 4.5 | 8 44.8 | + 1.1 |
| 29 | 6 13 12.7 | 38.5 | 1.05 | 8 20.8 | 8 12.4 | 8 16.2 | + 3.8 | 8 14.1 | + 1.8 |
| 23 | 6 30 26.4 | 58.3 | .98 | 7 46.4 | 7 57.5 | 7 57.1 | − 0.4 | 7 55 1 | + 1.7 |
| 7 | 7 6 8.5 | 33.7 | .91 | 7 31.4 | 7 19.8 | 7 20.5 | + 0.7 | 7 19.6 | + 1.7 |
| 24 | 7 22 13.6 | 61.8 | .88 | 6 58.0 | 7 11.5 | 7 6.0 | − 5.5 | 7 5.2 | + 2.3 |
| 13 | 8 20 21.3 | 39.9 | .82 | 6 36.5 | 6 22.9 | 6 20.8 | − 2.1 | 6 19.9 | + 2.0 |
| | ° ′ ″ | Int. | ″ | ′ ″ | Int. 50° | ′ ″ | ″ | ′ ″ | |
| Br.22 | 7 8 13 | 66.0 | .89 | 6 54.2 | 7 8.4 | 7 9.6 | + 1.2 | 7 8.8 | |
| P.156 | 13 43 0 | 45.0 | .47 | 3 57.0 | 3 54.6 | 3 54.6 | 0.0 | 3 54.9 | |

The mean refractions of the Nautical Almanac are here compared with the observations as reduced to the standard temperature of 46.°5 for the exterior thermometer, by taking a mean of the English and French corrections, for the reasons explained in the thirteenth number of these collections. Mr. Ivory not having precisely assigned the temperature at which his tables may be supposed to represent the results of the exterior thermometer, the errors of the New Tables have been copied from his own comparison of these tables with Mr. Groombridge's observations, and it is obvious that the New Tables, thus compared with the phenomena, do not at least possess any advantage over those of the Nautical Almanac; though they can scarcely be said to be decidedly inferior, with regard to the lower altitudes: but at 45° it appears to be highly improbable that the refraction, at the temperature of 50°, or even of 48°, can be so much as 57.″36.

Professor Bessel has given us, in the Berlin Almanac for 1826, the results of his latest observations on the refractions near the horizon, which show, very satisfactorily, what was perfectly well known before, that his table is founded upon an inadequate theory, and that it is of no particular use where any correct theory at all is wanted. The comparative results of the different determinations may be exhibited in a short table.

*Comparative Results, Barometer* 30.

| Alt. | N.A. 46°.5 Ext. 50° Int. | N. A. 52° Int. | Ivory | Diff. | Bessel F. A. | Bessel Corrected | Corr. |
|---|---|---|---|---|---|---|---|
| ° ′ | ′ ″ | ′ ″ | ′ ″ | ″ | ′ ″ | ′ ″ | ″ |
| 0 0 | 33 51 | 33 34.8 | 34 17.5 | +42.7 | 36 36.1 | 36 0 | −36 |
| 0 30 | 28 37 | 28 24.8 | 28 40.8 | +16.0 | 29 51.4 | 29 27.3 | −24.06 |
| 1 0 | 24 25 | 24 15.6 | 24 21.8 | +6.2 | 24 58.2 | 24 44.6 | −13.60 |
| 1 30 | 21 7 | 20 59.2 | 20 59.6 | +0.4 | 21 18.3 | 21 7.9 | −10.35 |
| 2 0 | 18 29 | 18 22.6 | 18 19.6 | −3.0 | 18 28.8 | 18 23.3 | − 5.46 |
| 2 30 | 16 21 | 16 15.6 | 16 10.89 | −4.7 | 16 15.1 | 16 13.9 | − 1.19 |
| 3 0 | 14 35 | 14 30.4 | 14 26.04 | −4.4 | 14 27.5 | 14 26.1 | − 1.42 |
| 3 30 | 13 7 | 13 3.0 | 12 59.51 | −3.5 | 12 59.5 | 12 58.7 | − 0.77 |
| 4 0 | 11 52 | 11 48.6 | 11 47.15 | −1.1 | 11 46.4 | 11 48.4 | + 1.97 |
| 4 30 | 10 50 | 10 46.9 | 10 46.03 | −0.9 | 10 44.9 | 10 48.2 | + 3.30 |
| 5 0 | 9 58 | 9 55.2 | 9 53.84 | −1.4 | 9 52.5 | 9 52.4 | + 0.05 |
| 6 0 | 8 32 | 8 29.7 | 8 29.80 | +0.1 | 8 28.5 | Berl. Astr. Jahrb. | |
| 7 0 | 7 27 | 7 25.0 | 7 25.40 | +0.4 | 7 24.2 | 1826. | |
| 8 0 | 6 35 | 6 33.3 | 6 34.68 | +0.4 | 6 33.6 | | |
| 9 0 | 5 54 | 5 52.5 | 5 53.79 | +1.3 | 5 52.8 | | |
| 10 0 | 5 20 | 5 18.6 | 5 20.19 | +1.6 | 5 19.4 | | |
| 15 0 | 3 34.3 | 3 33.4 | 3 34.70 | +1.3 | 3 34.3 | | |

| Alt. | N. A. 56°.4 Ext. 50°. Int. | N. A. 52° Int. | Ivory | Diff. | Bessel F. A. |
|---|---|---|---|---|---|
| ° | ′ ″ | ′ ″ | ′ ″ | ″ | ′ ″ |
| 20 0 | 2 38.7 | 2 38.1 | 2 39.16 | +1.2 | 2 28.9 |
| 25 0 | 2 4.2 | 2 3.7 | 2 4.65 | +1.0 | 2 4.4 |
| 30 0 | 1 40.5 | 1 40.1 | 1 40.85 | +0.7 | 1 40.7 |
| 35 0 | 1 23.0 | 1 22.7 | 1 23.25 | +0.5 | 1 23.1 |
| 40 0 | 1 9.3 | 1 9.0 | 1 9.52 | +0.5 | 1 9.4 |
| 45 0 | 58.1 | 57.9 | 58.36 | +0.5 | 58.27 |
| 50 0 | 48 8 | 48.6 | 48.99 | +0.4 | 48.91 |
| 60 0 | 33.6 | 33.5 | 33.72 | +0.2 | 33.67 |
| 70 0 | 21.2 | 21.1 | 21.26 | +0.16 | 21.23 |
| 80 0 | 10.2 | 10.2 | 10.30 | +0.10 | 10.29 |

The results of the Nautical Almanac are reduced to 52°, in order to compare them the more readily with those of Mr. Ivory.

For the mean probable error of a single observation, Professor Bessel and Mr. Rosenberger have found at

| Alt. | Error | Alt. | Error | Alt. | Error |
|---|---|---|---|---|---|
| ° ′ | ″ | ° ′ | ″ | ° ′ | ″ |
| 0 30 | 20.0 | 2 30 | 5.3 | 4 30 | 2.0 |
| 1 0 | 16.8 | 3 0 | 3.9 | 5 0 | 1.7 |
| 1 30 | 10.6 | 3 30 | 2.6 | 10 0 | 0.9 |
| 2 0 | 7.7 | 4 0 | 2.4 | 45 0 | 0.3 |

And the probable ultimate errors of their determination of the mean refraction, at 1° and 2°, are found to be 2″.5 and 1″ respectively.

With respect to the refractions *below* the horizon to which the table of Bessel extends, it will always be amply sufficient to take the *mean* horizontal refraction, and to increase it by its excess above the refraction computed for an altitude equal to the depression, and for the *actual* state of the atmosphere; except that if the temperature at the surface of the sea were known to be elevated or depressed, it would be proper to correct the mean horizontal refraction accordingly.

The whole of this comparison has been instituted in order to ascertain the propriety of retaining or suppressing the remark subjoined to the table in the Nautical Almanac, that it "appears to agree more perfectly with the latest observations than any other table that has been published," explained as it is by the admission in the preface, that the "deviation from the French tables in the mean value of the refraction," is altogether inconsiderable. If the new table had been decidedly shown to exhibit results more exactly conformable with the actual determinations of astronomers than those of the Nautical Almanac, it would have become the duty of the editor to adopt them; but for the present there can be no question that such a change would at least be premature.

### iv. *Note on* Refraction, *addressed to Professor* Schumacher.

My dear Sir,

I was much surprised the other day to observe, that in copying the explanation of my Table of Refractions from the Nautical Almanac, you had omitted, without assigning any reason, the words " which would be more consistent with the theory;" an expression which I had employed in speaking of the use of the external thermometer, in preference to the interior.

I am the more disposed to remonstrate with you on this occasion, because I observe that a great number of astronomers, and among them some who do not usually act without reflection, have inconsiderately taken it for granted that the correction ought to be made according to the height of the interior thermometer, as nearest to the place of observation.

Now, with regard to the theory, it is perfectly obvious, that the computation extends only to such changes of density as take place between the different strata of the atmosphere considered as *horizontal*; and that its results must necessarily terminate where this regular constitution of the atmosphere ends; that is, *outside* the observatory, or other building, containing the instruments; while the change of density between the external and internal air, taking place in general at surfaces more nearly vertical than horizontal, at least when the object is but little elevated, and certainly never at horizontal surfaces, will either have no effect at all in increasing the refraction, or as great an effect at higher as at lower altitudes; so that this little irregular addition or diminution can never require to be considered as a part proportional to the whole original mean refraction.

With regard to practice and observation, I need only refer to

Mr. Delambre's remarks in the *Connaissance des Tems,* for 1819, where he shows that for Mr. Groombridge's observations, the mean error of the exterior thermometer is only five sixths as great as that of the interior. * * *

### v. *General Results of Observations on the Dipping Needle.*

By W. Scoresby, Jun. Esq.

| 1823 | Time | Place | | Mean dip. |
|---|---|---|---|---|
| Mar. | 29 | Liverpool | | 71° 33′ 0″ |
| June | 10 | La. 71° 31′ 14″ N. | Lo. 12° 7′ 15″ W. | 78 3 6 |
| July | 5 | 71 38 0 | 17 37 0 | 79 0 7 or<br>79 6 7 (M.) |

The mean dip is the mean result of observations with and without a sphere on the needle: the last result, (M.) is obtained by the formula of Professor Mayer.

### vi. *Elements of the* Comet *of* 1823-4. *By various Computers.*

1. The first received by the Editor were from Mr. J. Taylor of the Royal Observatory, Greenwich.. 2. The second are by Professor Nicolai Schumacher, Astr. N. N. 48. B. 3; giving the greatest error in A. R. + 18″, in decl. + 11″. 3. The third by Mr. Hansen, A. N. 48, B. 3. 4. The fourth by Carlini. 5. The fifth by Dr. Brinkley. 6. The sixth by Mr. Richardson, of Greenwich.

| | | | |
|---|---|---|---|
| Passage of Perihelium | 1. | 1823 Dec. 9.3697$^{d}$ | Greenwich |
| | 2. | 9.4380 | Manheim |
| | 3. | 9.47193 | Altona |
| | 4. | 9.4792 | Greenwich |
| | 5. | 9.2168 | Greenwich |
| | 6. | 9.4521 | Greenwich |

| | | | | |
|---|---|---|---|---|
| Longitude of ☊ | 1. | 302° 56′ 34′ | 4. | 303° 4′ 4″ |
| | 2. | 303 1 18 | 5. | 303 0 40 |
| | 3. | 303 3 22 | 6. | 303 1 43 |
| — Perihelium | 1. | 28 43 54 | 4. | 28 26 8 |
| | 2. | 28 43 46 | 5. | 29 18 50 |
| | 3. | 28 29 55 | 6. | 28 20 6 |
| Log. nearest distance | 1. | 9.3598242 | 4. | 9.3545000 |
| | 2. | 9.3579600 | 5. | 9.3689400 |
| | 3. | 9.3553934 | 6. | 9.3536855 |
| Inclination . . . | 1. | 75° 55′ 45″ | 4. | 76° 12′ 50″ |
| | 2. | 76 9 40 | 5. | 76 1 40 |
| | 3. | 76 11 22 | 6. | 76 8 28 |

Motion retrograde.

# Art. XII. ANALYLIS OF SCIENTIFIC BOOKS.

*Sur les Ichthyolites ou Poissons fossiles. Par* Monsieur Blainville. *Article extrait du Nouveau Dictionaire d'Histoire Naturelle.* Vol. 28. *Paris*, 1822.

The reputation and the success of Cuvier in that department of Natural History which respects the animals of a former state of the globe, and his comparative omission of that branch of those researches which relates to fishes, seem to have stimulated the author now before us to the present undertaking. The ambition is laudable, but we fear that we cannot say the same of the execution. Though this is far from being the first work which has pretended to treat of fossil fishes, it is the first which has been exhibited as a complete system of the present state of knowledge on that subject. That it is both faulty and imperfect, is sufficiently obvious; and we are much inclined to think, that if Cuvier had considered the subject as capable of being undertaken to any purpose, he would not have left it to his ambitious imitator.

We fear we must say that this is an instance, among many now too common, of that desire to shine or glitter in a new science, which is the disease of the day, and which has rendered geology the victim as it is the butt of every tyro, who, incapable of dealing with the accurate sciences, hopes to acquire some notice by evacuating on the unlucky public, the records of observations and speculations which, like their authors, have scarcely cracked the shell, and are desirous of flying before they have learned to walk.

There are reasons however for our faith in all cases; and we owe Monsieur Blainville the politeness of shewing what his claims to authority on this question are. The work must rather be considered a compilation from the writings and observations of others, than an original production; and therefore the authorities are rather those of Scheuchzer, Haller, Volta, Faujas de St. Fond, Lametherie, Lamanon, Cuvier, and others, than those of the writer himself. Thus far, different measures of confidence will be allotted to them: confidence regulated, partly by the weight of the several persons quoted, and partly by the state of knowledge of the periods at which they severally wrote. Besides, and in aid of this, Monsieur Blainville has had access to the collections in the Museum of Paris, and thus has had the power of comparing, in many cases, the printed descriptions with the specimens.

But somewhat more than all this is required for a just or luminous compilation on such a subject; and here, our author, we are sorry to say, is entirely deficient. It is one thing, even admitting that could be done in the present case, to ascertain the comparative anatomy of the animals in question, and another to assign them truly to their geological situations. It is not sufficient that every fragment of bone, whether of land animals or of fishes, should be referred to an individual species or genus; but it is most essential that the true geological situations in which they occur, should be accurately understood and accurately described. We do not want to know merely what animals have existed, but when, where, and how they lived. Doubtless, it is important to know that many animals lived at all in a former state of the world, which are living in it no longer; it is important to know what these were, and how many; what species and genera have disappeared. This is a question however, which, abstractedly taken, concerns zoology only. The geologist is anxious for much more. He desires to know at what period of the globe they were in existence, in what lands or waters they lived, when they were buried and preserved, and how. And he desires to know all this, because he makes use of it as evidence respecting the history of the globe and its revolutions. Hence, he ascertains, or at least adduces collateral evidence towards ascertaining, the nature, and order, and places, and comparative times of its revolutions; and thus he acquires knowledge which, judiciously combined with the history of the mere rocks themselves and their various phenomena, enable him to make nearer approximations to a true theory of the earth.

It is indispensable therefore that the comparative anatomist should in this case be a practical, expert, judicious, and experienced geologist. He should be as replete with sound logic as he is free of system; should be as accurate an observer of geological facts as well stored with observations; and should be able, from his general knowledge, to exercise a critical and sound judgment on the reports and observations in zoology as in natural history, of those from whom he is compelled to borrow what he has not possessed the means of ascertaining from personal observation.

We wish we could say that Cuvier, much as we respect his soundness of mind and minute knowledge in comparative anatomy, were able equally to stand this test in geology. We wish we could say this in a far minor degree of Monsieur Blainville; but he is no geologist. Judging from his book, we are entitled to say that he has as little knowledge of this important part of his duty as is well possible. He is no observer, and he cannot be a critic. Hence every thing from which the geologist ought to have derived assistance, all that he would have turned to for light, only leaves him in darkness, the same or worse than before.

Monsieur Blainville copies, without discrimination, from the description of those who wrote before geology had been rescued from its ancient state of night and chaos; and, unable or unwilling to verify or rectify the observations of his remote predecessors, leaves every thing where he found it, or rather, adds to the confusion which pervades their remarks.

Such a work could not have been executed as it ought to have been, either in Paris or in the Paris Museum. It ought not to have been attempted, but by him at least, whose experience in geology rendered him competent, from other knowledge, acquired in other places, to verify the probable truth or detect the fallacy or imperfection of the reports of places which he was unable to visit. That the attempt has consequently failed in its most essential part, is but too plain.

We wish that what we have said, (and we might say much more did our limits permit,) would impress, not only on our neighbours but on the geologists of our own country, the necessity of keeping a steady regard, in their investigations, on the ultimate purposes to which these ought to tend or be directed. Geology itself, the history of the globe of the earth, is a difficult, severe, abstruse and laborious study. It requires much personal labour, much time, much acuteness, some reading, much freedom from system and prejudice, and an earnest desire for truth; with a cautious, rigid, severe, logic, and trained habits of a close and strict reasoning, which partakes often as much of moral and metaphysical, as of mathematical thinking and induction. It is not the collecting of specimens, or the forming of sections in the closet, and of coloured maps from the imagination, or from much conjecture and little observation, which constitute geology; and, this abstracted, there is little in it to satisfy the craving desire for ease and amusement united, and for some poor temporary fame to be acquired by papers in transactions and systems of Scotland or Siberia, on which the dilettantes in science live. Hence the labour is shunned; and the study evaporates in the far easier task of collecting bones and shells, in marvelling at the crocodile and rhinoceros which occupy the place since held by the two kings of Brentford, or at the hyena who proves the hardness of his jaws upon the bones of Yorkshire rats, and at the nature of *Album Græcum.*

But we must reserve our general criticisms on the present state of this science for a fairer opportunity, and return to Monsieur Blainville.

We have said that he was no geologist, and that he was incompetent to his subject, because deficient in that most essential part of it. But we have a serious objection also to make, to the other department of his work; to the rigidly zoological or anatomical part of it. All the world has marvelled, and with some reason, at the ingenuity with which Cuvier has contrived to erect new genera and species, and to produce entire animals which were never yet seen, and never will be, from fragments of rotten bones; constructing a Megætherium from a maxilla, and a Hyena from an os hyoides. With this we have nothing at present to do; satisfied with the ingenuity of the Zadig of the day, and, as far as authority can avail, quite as willing to allow him the dictatorship in this matter as any other person. There is always a latent delight in surrendering ourselves to the marvellous.

But even to this delight there are bounds; and when these are exceeded, we are apt to feel a twinge of the " incredulus odi." Unquestionably, the skeleton of a fish may be good evidence for the fish itself, as far as we may be satisfied without regaling on it, or are contented with guessing how it might have looked in a drawing, or skinned, varnished, and stuffed with plaster of Paris. This mode of assigning a species or a genus, will be still more satisfactory, when the naturalist has had the means of comparing the preserved skeletons of former days with those of existing species and genera to a sufficient extent. But who need be told that there is such a simplicity and general uniformity in the skeletons of fishes, that the limits to this mode of investigation are very narrow indeed. They have no legs nor arms, no scapulæ nor knee-pans, no os coccygis nor stermim, nor phalanges, nor any of those multitudinous and ever varied parts from which the comparative anatomist derives so much facility in his researches on quadrupeds. There is something in the number of the spinal bones, there is something in their forms and proportions; and there is still more in the bones of the fins and in those of the skull. But all this is little; and while but little evidence can be derived from fragments, we are particularly determined to distrust Monsieur Blainville on points which neither he nor any one else could have ascertained, namely, the erection of new genera and new species from the contemplation of fragments, and these fragments often distorted by the effects of pressure and the other causes of change and injury to which fossil bones are exposed.

When we said that Monsieur Blainville was ignorant of geology, we might also have said that he does not seem to have formed any conception of its nature and meaning, and of the relations of his own subject to it; considering this, as we do, rather a branch of zoology than of geology properly so called. He speaks as if the strata were only casual substances which might or might not be studied, but as being "often useful." We would gladly know how they are not always necessary instead of being often useful, at least in our view of the subject. If the object is merely to ascertain lost animals, they are neither necessary nor useful; whence it is plain that when M. Blainville speaks thus, he is thinking of geology, not of his fishes, and thinking too, to little purpose.

When he asserts that the nature of the organic remains offer the most "unequivocal methods of establishing geology on indisputable bases," he is only saying what others have said before him, but which is not a bit the more true because it has been often said. In the first place, we will admit this, and then ask to what extent the science of geology can be based on the knowledge of organic remains? In many countries they do not exist; in many rocks they never occurred. They are limited to a small portion of the geographical globe, and they are confined to a small depth of the geological one. What would become of the theory and history of the primary rocks, of the trap rocks, of the volcanic rocks, if their history and theory depended on their organic remains? The organic remains are connected mainly with the last revolutions of the globe;—universally with the later ones. And yet among these later, we must except the latest of all, which are the whole of the two classes of rocks produced by fire, the traps and the volcanic rocks. As to all the rocks which precede coal, with little exception, we should never obtain any knowledge of their theory, did we depend on the evidence to be derived from organized bodies. That they prove many things, is unquestionable: but it would be a defective system of geology indeed, and we might well despair of attaining any knowledge of that science, if we had no more knowledge and no wider views than Monsieur Blainville, and, (we might add,) many more, seem to possess on this question.

We are equally ready to deny, and to prove it, had we room, that even the order of the succession and the true theory of those very strata in which organic remains exist, can be proved by means of them. This has been a favourite theory to the present day, and it has a large body of abettors still. But we could prove, by their own evidence and shewing, that it is unfounded; by quoting their own catalogues of the strata and their included shells, and by shewing that the same genera, and the same species in many cases, occur through all the series, in positions the most remote. We could even prove it *à priori* from zoological considerations. Were the assertion true in geology, or in organic mineralogy, (to use a better phrase,) then it would have been a necessary preliminary that all climates should have produced, at different remote times, similar families of animals; that all these should have followed each other in a certain unvarying order, and that the same order and kinds should have existed and succeeded every where in one manner. It would have been impossible that there should now have been, had the same laws prevailed formerly as now, oysters at Milton, and muscles at Hastings, and cockles at Margate, and periwinkles at Dover.

But we have not time for what well deserves a separate discussion; and having thus far disputed Monsieur Blainville's preliminaries, shall proceed to make a few remarks and extracts from a book which we might have easily disputed at every page.

As we cannot afford to quote a great deal, we must try to be content with a few passages, and shall take the following in the first instance. We insinuated this author's want of logic; and surely it was an unjust insinuation, since the arrangement would do justice even to Jeremy Bentham. There is a Tudesqueness in it which is quite delightful, and which bespeaks the genius of a German professor crazed with the logic of Kant and Burgersdyck, and the reading of the schools, rather than the œstrus of a lively Parisian skipping through the dry bones of the Musée. If it is a long passage, we can only say in its defence, as Horace Walpole did after Gray, and of other passages, that it " leads to nothing."

"Sous le rapport de la composition chimique ou anatomique," (he is speaking of organic fossils.) J'ai divisé les corps organises fossiles ; A. en ceux qui n'ont éprouvé aucun changement dans leur tissu, dans leurs compositions chimique et mineralogique ; B. en ceux qui ont perdu seulement et entierément la matiére animale; C. ceux qui ont la même composition chimique moins la matiére animale, mais qui ont perdu de plus leur structure et leur forme; D. ceux qui n'ont perdu que la partie organique, mais dont la portion inorganique a pris une disposition toute différente *de la Spathification;* E. qui ont éprouvé des changements dans la tissu anatomique et dans la composition chimique, même dans l'acide du sel terreux qui les fermait ; F. ceux qui n'ayant rien perdu dans la structure organique. ont été entiérement changés dans la composition chimique; *de la Petrification:* G. fossiles qui sans avoir éprouvé de modifications ont été imprégnés d'une substance metallique ; *de l'Impregnation ;* H. des corps incrustés ; I. des corps succinisés."

This reminds us of a modern Act of Parliament: a trap set to catch all the modifications of possibility, and somewhat more; but which is still so ill-constructed, that lawyers, rats, and criminals contrive to escape it.

A German engrafted on a Parisian, forms a heteroclite-enough animal ; somewhat, we should conceive, like Monsieur Blainville's own palæobalistums and palæorynchuses. But such is the consequence of going to school at Freyberg. We thought that the Pope had been dead, for, like his namesake in John Bunyan, his nails had been pared some time ago ; but it seems that his ghost still walks. The earth, says our author, is divided and subdivided. This is highly instructive ; and firstly of the second, which are the organiferous strata, and of which the subdivisions are thus stated. We would have translated this logical and luminous passage for the benefit of our English readers but we want words. The divisions in French therefore are, " 1re. Terrains zootiques les plus antiques, trés antiques et antiques : 2me. Terrains les plus anciens, trés anciens, et anciens : 3me. Terrains modernes, comme d'alluvion, des tourbiéres : and lastly, 4. Terrains recens ou terrains meubles et couches superficielles." We have condensed the quotations, and hope they are the more intelligible. As to the first or the grand division, it is zootique and azootique. It is a fine thing to understand Greek and Logic ; and the author's positive, comparative and superlative, antic and ancient strata, remind us of the " heavy not particularly light," and the " intermediate between hard and semi-hard inclining to the soft," with the " scopiformly divergently radiated," and so forth, which argue the metaphysical and delicate profundity in language and thinking, which distinguishes another of the luminaries of this science.

But we have said enough of this author's general views, and must give a few specimens of his details. Of these, after the geological confusion which we have already sufficiently noticed, the leading character is the ambitious desire of creating new species and genera; apparently, with the design of rivaling Cuvier in his own peculiar walk, and for the sake of displaying his profound knowledge of Greek. As this language constitutes rather a novel science in France, we must excuse Monsieur Blainville for his wish to prove that he is actually the possessor of a Greek Lexicon.

Whether the strata of Glaris are to be considered as most antique or very antique, or antique, or whether they are most ancient or very ancient, or ancient, to which of these two sets, in short, of positive, comparative, and superlative entities they belong, Monsieur Blainville has not told us, and we cannot guess. But we must try if we can conjecture to what rank they belong in the vulgar language. We sincerely wish that geologists would use the same words as other people, to express such ideas as they happen to possess. If matters proceed much further in this way, what with German nomenclatures and French nomenclatures, books on geology will become as intelligible as the treatises for digesting sol with luna under the red dragon per "pemset remsen ame muc senvu saltrafi."

Scheuchzer, as well as Ebel and many others, have examined this place, and many collections have been formed from it. It is situated to the south-east of Glaris, in a small valley, at a distance of about five miles, in a part of the mountains called the Plattenberg. The including rock is a blackish fissile schist, containing some mica, and interstratified with thin laminæ of limestone. It is, as he says, the Grauwacké schiefer of the Germans, and, what is worse, the Phyllade pailletée of Brongniart, as if one hard name was not enough. The specimens here are very imperfect, being only the impressions of the fragments of skeletons; one side of which has formed a sort of bas-relief in the schist, while the other is very ill-defined.

Now Haller, a name not to be spoken of lightly, even by Monsieur Blainville, mentions the impressions of ferns as being found in the same places; but this he thinks improbable, because Brongniart found none in the collections which he examined, and chooses to call it a transition rock. Here the question of the geological nature of this deposit becomes most important. Our author, making up his mind that it is a marine formation, determines that all his specimens are sea-fishes. We have abundance of respect for Brongniart, but have also good reasons for not giving implicit credence to his geological opinions. Cuvier thinks it is marine, because it contains the remains of a tortoise, and because that tortoise must have been a marine one. We should be very glad to know whence this necessity arises: there was formerly the same compulsion on all the Lacertæ, the crocodiles, to belong to fresh-water; but, unluckily, Lieutenant Kotzebue finds that there are sea crocodiles in the Pellew islands. Here then we have a contest of evidence: the ferns which Haller saw, against the tortoise that *must* have lived in the sea; and, further, the opinion of Andræa, who says that it is a fresh-water formation, against that of Brongniart. We do not intend to decide between disagreeing doctors, but it is a justice to our readers, if they are readers also of Monsieur Blainville's Ichthyolithology, to dissect these paragraphs for their use.

Eight species of fish are described as being found here. Of two of these, Monsieur Blainville makes new genera, by the names of Anenchelum and Palæorhynchum; the others are supposed to belong to Clupea and Zeus. The first of these was formerly imagined to be an eel; and although that opinion was probably wrong, we cannot see, how, from the miserable evidence which the fragments are admitted to afford, it is possible to make a new genus for it. But this naturalist finds less difficulty in constructing a genus out of a fin or a tail, than Linnæus did with the whole living races before him: according to the well-known adage, "Qui ad pauca respiciunt de facili judicant." Here is the way, for example, in which Palæorhynchum (old Snout, for the benefit of the unlearned) is made. "Quoique cet Ichthyolite, dont nous n'avons vu que *la figure de la partie anterieure*, dans l'Herbarium diluvianum tab. 9, fig. 6, nous soit trop insuffisament connu pour appuyer notre opinion, il ne nous paroit nullement probable que ce soit notre aiguille; (Esox bellone,) ainsi donc, jusqu' à des circonstances plus favorables, nous proposerons de la designer provisoirement sous le nom de Palæorhynque de Glaris." We shall really be glad to know how such trifling as this can conduce to the study of ichthyology, or geology, or any other Ology in the whole circle of the sciences.

Scheuchzer takes another of these fishes for a bleak; not an unlikely conclusion, if this same deposit contains ferns; but our author chooses to make it a new clupea. A fourth was esteemed a pike, and this also he makes a clupea; which judgment being deduced, not from a specimen, but from a figure by Knorr, it is very satisfactory to be informed that it is uncertain whether that appertains to the rock of Glaris or not. On such principles as this, we are likely soon to abound in Ichthyolitologists and Ichthyolitologies. There is a third clupea, with a new title also; all of which is matter of course, since it was predetermined that this was a marine formation.

Next comes the genus Zeus, of which he finds three new species. To show how satisfactorily these points are settled, in the first place, the first species is determined to belong to Glaris, not because it was found there, but because it lies in a similar slate: as if similar rocks of all kinds were not found all over the world. This may very well be a marine fish, if he pleases; but how does it follow that it has any thing to do with Glaris, or that it proves this to be a marine deposit? As to its own characters, it has "des rapports avec le Zeus, ou genres voisins; mais c'est ce qu'on ne peut assurer, parce que la tête toute entiere manque." Then "toujours est il constant que c'est un poisson marin." Very possible. And so for the others.

The fossil fish of what is called the metalliferous slate, are well known to be abundant, in numbers at least, if not in kinds; and they occur in many different places. The most noted of these are the Palatinate, the Voigt, and Thuringia; and they have often been described by different authors, such as Kruger, Friesleben, and others. It is somewhat remarkable with respect to these specimens, that they are almost always much distorted and injured; not even being compressed and preserved laterally, as is the most usual case in the fossil fishes. It is equally so, and important at the same time in investigating the species, that the impressions are those of the skin and substance of the animal, not of the skeletons.

With respect to the nature of this deposit, we have no objection to be convinced that it is a marine one, if it appears that any one of the fish is marine, or that any one sea-shell is contained in it. It may very probably be so, although no such evidence is produced. But if we are willing to believe quietly and without any evidence at all, we do not choose to be obliged to believe by that which is not evidence; and this, not on account of any concern we feel about the bitumino-metalliferous schist of Mansfeld, but because of the very testimony itself. We have a mortal aversion to corrupt evidence in all its modes, and do not choose to pass any attempts to introduce any more of it into geology, where there is already an abundance.

These strata are determined to be marine because they lie beneath calcareous rocks containing "ancient" (or modern) marine shells, such as belemnites, entrochites, and ammonites, of the same kind as those that belong to the limestones of the Apennines and Alps, together with gypsum accompanied by sea-salt, gypsum without sea-salt, sandstone, and so forth. Now these strata are the exact counterparts of the red marl and lime of England; particularly where they are somewhat intermixed. We have no objection, either to their marine origin, or to their antiquity, if that will give Monsieur Blainville any satisfaction; but neither of these will prove that the strata below them are of marine origin also. Our own coal strata are situated in this very position; and no one now, it is hoped, since the theories of Dr. Hutton and of Kirwan on this subject are forgotten, will imagine that a series which contains terrestrial vegetables in abundance, and which never was known fairly to include a sea-shell, is of marine origin. Thus much for what is possible respecting this deposit of ichthyolites.

Monsieur Blainville has made twelve species and three new genera out of this collection. The new are palæoniscum, palæothrissum and stromatcus: the old ones clupea and esox. In general we may remark on these determinations, that they are more free from objections than some of the preceding; as the author had access, in many instances at least, to more perfect specimens. How far, however, his arrangements are justified, we cannot pretend to decide. We may nevertheless remark, that he has here attempted to give generic characters, which, in some other cases, he has oddly enough seemed to have thought unnecessary. Surely if a genus or a name is to be erected for an ichthyolite, or any other kind of lite, the purpose is, if there be any purpose at all, to allow others to refer to it, and to arrange their discoveries under the appropriate division. If this is not to be, if there is only to be a palæo—something, without characters, we do not see what natural history, ichthyology, or geology is to profit by such a coinage of crabbed words; while we do see that, on the same principle, we may shortly have as many genera as there are specimens; a proceeding likely to be attended with no convenience that we know of to compensate for the vexation of such a catalogue, except that of inducing gentlemen to turn the leaves of the long-forgotten Lexicon; that unlucky book, thumbed in the Anglo Greek division by every projector who wants to dazzle our understandings with a Diatalaiporou, a Therapolegia, an Anthropomonotroche, or an Apolepsia alexicacon.

Of these new genera we shall give the characters of that in which our author has been most successful and appears most justified, as a compensation for some of the others which we have noticed. "Palæothrissum." "Il a pour caractere essentiel: d'etre abdominal, malacopterygien, de n'avoir qu'une seule nageoire superieure située devant l'anale, entre les pelviennes et elle, et surtout d'avoir la queue bifurquée, et le lobe supérieur ordinairement beaucoup plus long que l'inferieur, et couvert d'ecailles dans toute sa moitié supérieure." There are four species of this: but Kruger thinks that one of them is a pike; it is doubtful if two of the others are not the same, though each one has its own name; and, what is worse, the geological relations of the rock in which these are found is doubtful.

That one which follows is called stromateus; but, as is most usual, has no generic characters assigned. How species are to be established before a genus is determined, rather surpasses our comprehension; nevertheless there are three, with the names major, gibbosus, and hexagonus, (surely the love of arrangement is a terrible disease,) and two more without names, the genera of which, are, however, left in doubt. We shall extract a few words from these descriptions that naturalists may see what marks they may have to deal with when they take to the investigation of ichthyolites.

No. 11.—On trouve encore à Eisleben une autre espèce d'ichthyolite, mais qui jamais, ou fort rarement est entière: d'après la grandeur de sa tête, on suppose quelle pouvoit avoir près de trois pieds: sa peau etoit, dit on, comme chagrinée. D'aprés cette indication, je supposerois volontiers que les oryctographes indiquent par la un poisson fossile, dont j'ai vu l'empreinte d'une partie de la peau dans la collection de Monsieur Brongniart. On y peut reconnoitre à ce qu'il m'a semblé, une assez grande nageoire dorsale; mais ce que cette peau offre de remarquable, c'est d'etre entièrement récouverte d'espèces de petites écailles, comme trifurquées a leur pointe, et qui semblent formées par deux chevrons disposés en sens inverse.

Je n'essaierai aucune conjecture sur le genre de poisson à laquelle cette peau a appartenu; mais je ferai l'observation que l'espèce d'ecusson qu' on voit souvent à la racine des nageoires, dans les fossiles que j'ai designés sous le nom de Palæothissum, ressemble beaucoup a ces sortes d'ecailles."

We have no objection to this manner of contemplating the subject. It is proper that specimens, be they never so imperfect, should be preserved, and figured, and described; because by the comparison of fragments at some period, a species or a genus may really be determined: it is not often that our author is so moderate: and, to continue, we shall give his equally prudent remarks on No. 12.

"Enfin, on cite encore, comme d'Eisleben, quelques restes, dont la peau est lisse comme celles des anguilles. Je crois avoir vu, dans la collection de Monsieur Brongniart, l'empreinte d'une portion de peau, qui a du appartenir à cette espèce. Le peu que j'en ai observé, et qui me paroit provenir des environs de l'anus, indique évidemment un poisson anguilliforme: toute la partie superieure offre des stries nombreuses verticales; et l'autre moitié ou inferieure, est couverte de trés petites ecailles, fort luisantes, serrées, ovales, qu'on ne voit aisêment qu'à la loupe."

So much for the fishes of this deposit. But we must add that Leibnitz thought that he had found in it a mullet, a perch, and a bleak; Kruger also describes a pike; so that it may yet be a doubt whether these are marine or fresh water fishes; because, even if we were to grant Mons. Blainville all his new genera and species, it does not at all follow that they are marine ones. A word or two on this part of the subject will not be misplaced; as the determinations of our modern zoo-geolists on many parts of their investigations are very mainly and materially guided by certain notions which they have formed respecting the distinctive character of marine and fresh water species.

Let us put the very simple case that the salmon, the sea-trout, the sturgeon, or the sterlet, were found in a fossil state, we should be very glad to know how it is to be determined whether these are marine or fresh water fishes; they are both the one and the other.

But we will carry the matter a little further and say, that there are no marks in the anatomy or natural characters of a fish by which its habitation can be known *à priori*. It is a pure matter of experience now; and there is no experience about these ancient animals. For any thing to the contrary that we can ever hope to prove from natural characters, these ichthyolites may have been the inhabitants of fresh waters, or of the salt ocean, or of great inland lakes, such as are the lakes of Switzerland now, or such as the basin of Paris assuredly was long ago. In the same way, geologists have attempted to decide upon fossil shells, inhabitants of the sea and similar objects, the natives of fresh water. That also in matter of experience, and of that only; and of the past, the so long past, there can be none. There is no character by which these can be recognised: it is not to be found in their tenderness, or the reverse, as once imagined. It is not found in generic characters, because there are species in one genus, some of which are inhabitants of the sea, and some of lakes and rivers, just as much as there is a sea eel and a river eel; a Muræna anguilla and a Muræna conger. Indeed with respect to the shell fishes, Mons. Freminville has lately shown that sea and fresh water kinds all live together in the same place. But we need not pursue this point further, and shall return with Mons. Blainville to his next geological division, the ichthyolites of what he calls the "Calcaire compacte."

As his method of division is geological, we think it would have been as well if he had satisfied his readers first of the propriety of his geological arrangements. "Calcaire compacte" may mean a great deal. The geological characters of the former strata, were merely doubtful: those of the present cannot possibly be right. The first locality, for example, is Granmont, situated at four leagues from Beaune in Burgundy; and the rock is the "calcaire ancienne, contenant des gryphites et des belemnites," which is "situé audessus du gres rouge, et que l'on croit presque aussi ancien que celui du Jura." The next is Italy, where, without any other evidence than the colour and look of the detached stone, one is decided to belong to the Apennine limestone; we have no hesitation in admitting that some of them actually do so.

If "calcaire compacte" is to comprise such rocks as these, and if it is thus to be considered as one geological formation, we ought to have been furnished with more accurate geological information respecting them, that we might have judged of the propriety of this arrangement. If there is any object in dividing the ichthyolites according to the strata in which they are found, it is for the purpose of inquiring into the somewhat interesting question of their relative antiquity. This is what Cuvier has properly done with respect to the Paris district; and Mons. Blainville, while he was about it, might as well have imitated him in that too, had he not been solely occupied on fish bones, thinking, doubtless, "in tenui labor at non tenuis gloria." But we must inquire about the Ichthyolites themselves.

There is first a new Elops, the macropterus, from Granmont, which may or may not be an Elops; and then there is another called incognitus, imbedded in a "pierre calcaire dure, assez compacte, grise, et formant une sorte de noyau dont j'ignore la localité et le gissement." This is not a very accurate geological arrangement at any rate. The fish of Italy are left pretty nearly as they were found, but our author takes, or makes, an opportunity of cutting off Brieslak's head with a golden hatchet. We cannot pardon Brieslak any more than M. Blainville, because he has a troublesome way of thinking for himself, and of professing that he does not understand the mystical language in which the French geognosts shroud their oracles:—"Brieslak fait l'observation, qui ni a été confirmée par Mons. Menard de la Groye, que on ne trouve dans cette localité qu' une seule espece de poisson fossile, que l'on regardé à Naples, et même parmi les savans, comme analogue du sparus quatracinus, appelé dans cette ville, sparaglioni. Comme l'observateur dont je viens de parler, en homme qui sait agir dans ces sortes de recherches, a raporté à la fois ce fossile et l'analogue présumé, j'ai pu, grâce a sa complaisance, m'assurer que ce rapprochement est tout-a-fait erroné. En effet, le poisson fossile me paroit appartenir au genre Zee, ou à l'une des subdivisions qu' y a introduites Mons. La Cépede; aussi la hauteur de son corps surpasse la moitié de sa longueur, tandis que, dans le sparus quatracinus, elle est environ le tiers." So much for Breislak and his sparaglioni.

The next geological formation is the chalk, which includes Brussels, Maestricht, Paris, and Perigueux. The first ichthyolite mentioned affords a good specimen of ichthyolitologistical reasoning. M. Burtin begins by giving "des figures assez bonnes." M. Blainville "n' en a pas vu lui meme," therefore it is, first "Zeus auratus?" "que je croirois volontiers du genre Pleuronecte, et peut être la Barbue, ou mieux encore le poisson de St. Pierre." John Dory after all. But then M. Burtin, who has drawn this very "figure assez bonne," sees fins, and ears, and sculls, and jaws, and teeth, and orbits, and clavicles, and scapula, and vertebræ; while M. Blainville sees "rien de tout cela dans la figure." And then M. Burtin "veut je ne sais trop pourquoi," that this is a Chætodon. But enough of the chalk formation. There is more of the same kind of useful information respecting the "formation du Calcaire grossier, inférieur au gypse." Cuvier seems to have been too wise to attempt it, and we shall spare our readers the sparus that may be a labrus.

Then succeeds an account of the fishes of Pappenheim, but we cannot afford to enter on the details in the same manner. The only remark we shall indulge ourselves in making, is, that in describing five species in the genus clupea, which seems a particular favourite with our author, he has borrowed from Knorr's figures, instead of consulting the specimens themselves. Thus the probabilities in favour of truth are, that, in the first place, Knorr himself is correct; next, that his painter has figured impressions of fish bones so accurately, when the value of the subject was not understood, as to enable M. Blainville to determine different species of clupea, and the genus itself from them; and, lastly, that the author has no favourite system respecting his genera, the contrary of which is evinced in every page of his work. The proof of this latter is, that all the figures which do not chance to suit the fashion of the moment, are pronounced bad; and that when they happen to suit it, they serve the purpose, with him, of demonstrating what such figures are totally incapable of proving.

His rage for maintaining his system at all hazards, is equally proved by another decision in this very case of Pappenheim. He finds a figure in Knorr, but without a locality assigned; yet he determines that it must belong to this spot, because it seems to lie in a similar stone. We should be glad to know how any figure can represent a stone, so as to render its locality certain, or even probable. We are sure that no figure of Knorr's is capable of distinguishing any one stone from another, far less the slates of Pappenheim from the slates of Shropshire or America. If thus M. Blainville's geology is studied and ascertained, we cannot have too little of it. As to the general geology of this celebrated spot, it is derived here from the description of Reuss and Humboldt; and a worse piece of geological observation and reasoning, we will venture to say, was never printed. That it is a fresh water formation appears almost certain, so far from being what is represented; and that the observers have confounded and misrepresented the relative positions of the fresh water and marine strata, is equally so, though we cannot here enter into the reasons for this opinion.

As to the ichthyology, it is of a piece with the geology, which is less pardonable, since the author's claims in this department are more decided. The figure is that of a sturgeon, and yet he chooses to decide that it is an unknown pike, to which he gives the name of Esox acutirostris. Stromatæus and Pæcilia, from the same place, are determined on grounds as slender. The specific name of the latter is Dubia; and if we were inclined to make a very low jest, we should say that it was applicable to three fourths of the whole collection.

Mons. Blainville has entered into considerable length on the subject of the celebrated fishes of Monte Bolca, and we are glad to say that in this part of his treatise he has been of real service to the cause in hand. It is, in truth, the most valuable, and we had almost said the only valuable part of his book. With the double advantage of the splendid work, published at Verona under Volta's direction, and of the collection itself procured, (plundered, as Ttaly asserts,) from Count Gazzola, he has been enabled to rectify the more glaring errors of the Italian naturalists, and to give somewhat like a rational list of the specimens. Out of Volta's forty-four genera, including a hundred and five species, he has admitted only ninety-three species, and it is quite plain that they would allow of still further purification.

This subject, however, is so extensive that we dare not enter it. We have no room for a criticism on genera and species, which indeed could not be rendered intelligible without the figures. But we are bound to say, on the geological question, that the Italian theory which collects these fishes from all parts of the world, is purely gratuitous; and thus while it is geologically impossible and groundless, it is contradicted icthyologically by the specimens themselves, which are now, in part, and in former times have probably all been, natives and residents of that sea which now washes the land in which their remains are preserved. It would be easy to state a rational theory of this formation, and its relations to parallel phenomena in many parts of the world: but they are in a good measure superseded by the present more rational views of the history of the supramarine organiferous strata.

We said before, that it was incumbent on an author, professing to give a general treatise on ichthyolites, or of any other branch of this science, to make himself acquainted with the facts at least which are accessible, or to acquire such knowledge as would enable him to profit by the observations of others. The defect of our author in these respects is peculiarly sensible in what follows, where he has copied careless observations in a careless manner, and rendered the confusion more offensive and troublesome by the systematical and decided form in which he has placed it. Mount Lebanon, Cerigo, Antibes, and many other localities are discussed in this slovenly manner, from Faujas de St. Fond, and others; and where we ought to have certainty we have only useless guessing.

We consider that an author who thus professes to write a systematical work, is bound to make it really systematical, as far as that is possible. It is a different thing to write single essays, or to describe those separate localities and partial facts from which geology ultimately derives assistance towards its general views. Hence our author is equally deserving of censure, that when he quotes Sicily, Malta, Iceland, and other well known countries, where fossil fishes have been found, he is scarcely ever at the trouble of ascertaining what has already been written about them, or of trying to extract something like truth from a balance of testimonies. As a specimen of this unpardonable carelessness, he quotes Antibes as a locality, and then doubts whether it is not Antigua. Nor could any thing but the same ambition to make a book and a system, which has led him to give genera without descriptions and species, under such imaginary and nominal genera, have tempted him to muster in his arrangement the fossil fishes of China, of which he knows nothing. On those of England he is equally unsuccessful; whereas he might have found something to his purpose, had he taken the trouble to seek for it.

We have already shown our suspicions that many of Mons. Blainville's marine formations, and marine ichthyolites, are really fresh water examples; but we have a detail in the latter part of his essay of those which are indisputably such, and which he chooses to call Potamiens, "apparemment," because Ποταμος is a river, and that these strata have been produced in lakes.

We may pass over the Italian examples, as unsatisfactory: those of France are better known, and are here better described. The deposit of Aix is well known to consist of five marked beds, reaching to a depth of near sixty feet, consisting of marl, limestone, bituminous marl slate, and gypsum. The fishes which it contains are one species in the genus perca, a cyprinus, and the mugil cephalus or grey mullet. This latter is a sea fish, and an inhabitant of the Mediterranean ; though, as we formerly remarked, it can live in fresh water ; and since this is a decided fresh water formation, here is a remarkable fact in proof of what we have already advanced on the uncertainty which attends this subject, and which, if Mons. Blainville had not been so strenuous a theorist, might have led him to be more cautious in many of his decisions on this subject.

The basin of Paris has been so thoroughly described by Cuvier and Brongniart, that little has been left for our author to do; and that description is also known to every one sufficiently, to render it unnecessary for us to enter into any details respecting it. These specimens are not numerous, and they are generally very imperfect and ill preserved. As our geological readers must know, they have been described by Lamanon, Faujas de St. Fond, and de la Metherie, as well as by Cuvier. The species are limited to seven, and they are all so ill defined that no very satisfactory conjecture respecting them has yet been made. We shall not quote what has been said, as it is of no moment in the present review of Mons. Blainville.

Yet we must be indulged in one remark on Cuvier himself in this case; professing, at the same time, that respect for his attainments which it is almost superfluous to profess. When the fishes of Vestenu nuova were first described, it was the fashion to suppose that the world had been turned upside down, and inside out, and if there were two ways of explaining a fact, it seemed to be the fashion and the ambition to reject the easiest and most natural solution, for the sake of adopting what was marvellous, incredible, or impossible. This has indeed been one of the leading diseases of geology and geologists.—Because it was impossible that obsidian and pumice could be formed by water, they were to be aqueous productions: because the identity of volcanic rocks and trap rocks was so absolute that we could almost suppose we had seen the latter formed by the same class of fires which produced the former, it was resolved that they were generated from water. Thus, at Vestenu nuova, because there was no difficulty whatever that the crowd of fishes which inhabit, or inhabited, the Mediterranean should have been elevated from the bottom of the sea, entangled in its mud, and indurated in rock, just as they have been before our very eyes in Iceland, it became necessary to collect them from the four quarters of the globe. The simple solution was not marvellous enough, and the dreams of Volta and his party have been triumphantly repeated and re-echoed, in our own clearer day, by those who prefer doubt and difficulty, to conviction and facility, and would rather that truth should not be attained than attain it by the easy road which all may apprehend.

Of this, we fear, we must accuse Cuvier himself in the case of Pæcilia vivipara, (as he chooses to suppose it,) of the Paris basin, a fish figured by Bloch, and a native of Surinam. For what possible purpose should we resort to Surinam for a fish for this situation? The theory is as purposeless as the voyage of the living fish itself to th Seine would be at this day, unless his object were to attract the praises, in a Bechamel sauce, of the Gastronomes who sit in judgment at the tables of Beauvilliers. The very theory of the Paris basin, to speak seriously, renders this supposition nearly as impossible as it is unreasonable, and it would surely be a more rational conclusion that the fish in question was either a lost native of the Parisian seas, or that the imperfection of the specimen was the cause of a resemblance far too slight and doubtful to give the slightest justification to such a useless and violent supposition.

We are fully aware at the same time of the argument in favour of such a view, which may be founded on the existence of vegetables with intertropical characters or analogies, in the same climates in which the Paris basin is situated. But this whole question, as far as it relates to change of climate, or an alteration in the position of the earth's axis, is very obscure, or more than obscure: and were it not so, it must be remembered that the coal strata belong to a far remote period of the earth, antecedent by many and by millenarian revolutions to the basin and deposit in question. We have no right to argue thus, and it is only to perpetuate the vice from which geology has already suffered so severely.

But it is time for us to bring this article to a close, and to take our leave of Mons. Blainville and his ichthyolites. We wish that we could have spoken more favourably of a performance which contains far more of conjecture and trifling than of useful and solid information, and which is not calculated to add much to our stock of knowledge. We do not undervalue this particular pursuit; on the contrary, we think it highly desirable that every organic fragment of a former world, in every department, should be collected, studied, and described: but geology, geology itself, the history of the structure and revolutions of the earth, has also its claims; and such collections and systems more than double their value when they are caused to bear and throw light on this important subject. This is what Mons. Blainville has yet to learn. We still hope and expect that he will look at his subject in this view; that he will turn from the poor ambition of shining in a catalogue of new and useless names, to that of improving the sciences which he has undertaken; and that, substituting study for guessing, and close investigation and careful reasoning for compilation and catalogue, he will appear before us again, at some future day, a new man, to receive the praise which we shall give with far more pleasure than we have passed the censure.

## II. *The Philosophical Transactions of the Royal Society of London, for the year* 1823. PART. II.

### 1. *On a New Phenomenon of Electro-Magnetism. By* Sir Humphry Davy, Bart. Pres. R.S.

This is a contribution of a curious fact to the new and interesting science of electro-magnetism, and it is by such contributions alone that this infant science can, at present, be expected to make any progress to maturity. Sir H. Davy found, that when two wires were placed in a basin of mercury, perpendicular to the surface, and in the voltaic circuit of a battery with large plates, and the pole of a powerful magnet held either above or below the wires, the mercury immediately began to revolve round the wire as an axis, according to the circumstances of electro-magnetic rotation, discovered by Mr. Faraday. Masses of mercury, of several inches in diameter, were set in motion, and made to revolve in this manner whenever the pole of the magnet was held near the perpendicular of the wire; but when the pole was held above the mercury, between the two wires, the circular motion ceased, and currents took place in the mercury in opposite directions, one to the right and the other to the left of the magnet. Other circumstances led to the belief that the passage of the electricity produced motions independent of the action of the magnet, and that the appearances were owing to a composition of forces.

The form of the last experiment was inverted, by passing two copper wires through two holes, three inches apart, in the bottom of a glass basin; the basin was then filled with mercury, which stood about the tenth of an inch above the wire. Upon making a communication through this arrangement, with a powerful voltaic circuit, the mercury was immediately seen in violent agitation; its surface became elevated into a small cone above each of the wires; waves flowed off in all directions from these cones, and the only point of rest was apparently where they met in the centre of the mercury, between the two wires. On holding the pole of a powerful magnet at a considerable distance above one of the cones, its apex was diminished and its base extended. At a smaller distance, the surface of the mercury became plane, and rotation slowly began round the wire. As the magnet approached, the rotation became more rapid; and when it was about half an inch above the mercury, a great depression of it was observed above the wire, and a vortex which reached almost to the surface of the wire.

The President thinks that these phenomena are not produced by any changes of temperature, or by common electrical repulsion, and concludes that they are of a novel kind.

## 2. *On Fluid Chlorine.* *By* M. Faraday, Chemical Assistant in the Royal Institution.

[Communicated by Sir H. Davy, Bart., Pres. R.S.]

This paper describes Mr. Faraday's first step in the important series of experiments, which led to the condensation of the gases. He prepared some dry hydrate of chlorine, at a low temperature, and introduced it into a glass tube, which was hermetically closed. Being placed in water at 100, the substance fused, the tube became filled with a bright yellow atmosphere, and on examination was found to contain two fluid substances: the one was of a faint yellow colour, and the other a heavy bright yellow fluid, lying at the bottom of the former, without any apparant tendency to mix with it. This fluid was easily distilled in a bent tube, and separated from the former. When the whole was allowed to cool, neither of the fluids solidified at a temperature above 34°, and the yellow portion not even at 0°. When the two were mixed together, they gradually combined at temperatures below 60°, and formed the same substance as that at first introduced. If, when the fluids were separated, the tube was cut in the middle, the parts flew asunder with an explosion, the whole of the yellow portion disappeared, and there was a powerful atmosphere of chlorine produced. The pale portion remained, and proved to be a weak solution of chlorine in water.

The result of this experiment was confirmed by condensing perfectly dry chlorine by a syringe, and then exposing it to a low temperature; it was thus readily made to assume the liquid form.

Fluid chlorine appears very limpid and fluid, and is excessively volatile at common pressure. Upon cooling a portion to 0° and then opening the tube, a part immediately flew off, leaving the rest so cooled by evaporation as to remain a fluid under the atmospheric pressure. Mr. Faraday thinks that the temperature could not have been above –40° in this case.

He calculates the specific gravity of fluid chlorine at 1·33.

In a note to this paper, the President of the Royal Society shews that these results will evidently lead to other researches of the same kind, and mentions, that by sealing muriate of ammonia and sulphuric acid in a glass tube, and causing them to act upon each other, he had procured liquid muriatic acid.

## 3. *On the Motions of the Eye, in Illustration of the Uses of the Muscles and Nerves of the Orbit.* *By* Charles Bell, Esq.

[Communicated by Sir H. Davy, Bart., P. R. S.]

This is a highly interesting paper, and, together with the second part, inserted in another part of the volume, is calculated to explain many ill-understood points of the mechanism and functions of the eye, and to renew our wonder at the properties of the organ itself, and the frame-work and apparatus by which it is suspended, moved, and protected. Mr. Bell concludes from his researches, that the high endowments which belong to this wonderful structure depend not exclusively, as is generally conceived, upon the ball and optic nerve, but upon its exterior apparatus also. It is to the muscles, and to the conclusions, we are enabled to draw from the consciousness of muscular effort, that we owe that geometrical sense by which we become acquainted with the form and magnitude and distance of objects. It is impossible to do justice to Mr. Bell's views in the short space to which we are obliged to confine ourselves in these abstracts: a careful perusal of the whole paper is absolutely necessary to those who would wish thoroughly to understand the investigation, and will amply repay even the more general reader. The author has shewn, by the most satisfactory illustrations, that we must distinguish the motions of the eye according to their objects or uses, whether for the direct purpose of vision, or for the preservation of the organ; that the eye undergoes a revolving motion not hitherto noticed; that it is subject to a state of rest and activity; and that the different conditions of the retina are accompanied by appropriate conditions of the surrounding muscles; that these muscles are to be distinguished into two natural classes; and that in sleep, faintness, and insensibility the eye-ball is given up to the one, and in watchfulness and the full exercise of the organ, it is given up to the influence of the other class of muscles; and, finally, that the consideration of these natural conditions of the eye explains its changes as symptomatic of disease, or as expressive of passion.

### 4. *An Account of an Apparatus on a peculiar Construction for performing Electro-Magnetic Experiments.* *By* W. H. Pepys, Esq., F.R.S.

### 5. *On the Condensation of several Gases into Liquids.* *By* M. Faraday, Chemical Assistant in the Royal Institution.

[Communicated by Sir H. Davy, Bart., P.R.S.]

In this paper, Mr. Faraday follows up the train of investigation which the condensation of chlorine, by its own elastic power, so obviously opened. Mercury and sulphuric acid were sealed up in a bent tube, and being brought to one end, heat was applied, whilst the other end was preserved cool by wet paper. The sulphurous acid, which was generated, passed to the cold end, and was condensed into a liquid. The properties of liquid sulphurous acid are as follow:—It is limpid and colourless; its refractive power about equal to that of water; it does not congeal at a temperature of 0°; its specific gravity is nearly 1·42, and it exerts a pressure of about two atmospheres at 45°.

Sulphuretted hydrogen was generated and condensed in an analogous way, from muriatic acid and sulphuret of iron. It was colourless, limpid, and excessively fluid. It was not rendered more adhesive by a temperature of 0°; its refractive power appeared to be rather greater than that of water, and the pressure of its atmosphere at 50, was equal to about 17 atmospheres. Its specific gravity about 0·9.

Carbonic acid was also condensed, but it required great precautions to effect the condensation with safety. It is a limpid, colourless body, extremely fluid, and floated, as did all the preceeding liquids, upon the contents of the tube, without mixing. It distils readily at the difference of temperature between 32° and 0°; its refractive power much less than that of water, and its vapour exerts a pressure of 36 atmospheres at a temperature of 32°. In endeavouring to open the tubes which contained it at one end, Mr. Faraday states, that they uniformly burst with powerful explosions.

Fluid enchlorine was also obtained, and proved to be a transparent substance, of a deep yellow colour, and highly elastic powers.

Liquid nitrous oxide is limpid and colourless. It boils rapidly by the difference of temperature between 50 and 0°, and does not solidify at – 10. Its refractive power is less than that of any known fluid, and the pressure of its vapour is equal to above fifty atmospheres at 45°.

Liquid cyanogen is limpid, colourless, and very fluid, and does not alter its state at the temperature of 0°. Its refractive powers rather less than that of water; its specific gravity nearly 0·9, and the pressure of its vapour about 3·7 atmospheres.

Mr. Faraday obtained dry ammonia from chloride of silver saturated with this gas*, and, by the usual process, succeeded in condensing it. It was colourless, transparent, and very fluid. Its refractive power surpassed that of water, and all the other liquids hitherto described. The pressure of its vapour is equal to about 6·5 atmospheres at 50°, and its specific gravity is 0·76. Attempts were made to obtain hydrogen, oxygen, fluoboracic, fluosilicic, and phosphuretted hydrogen gases in the liquid state; but though all of them have been subjected to great pressure, they have as yet resisted condensation.

### 6. *On the Application of Liquids formed by the Condensation of Gases as Mechanical Agents. By* Sir Humphry Davy, Bt. Pres. R.S.

In this paper Sir H. Davy anticipates the probability of the application of the elastic force of compressed gases to the movement of machines. He founds this anticipation upon the immense differences between the increase of elastic force in gases under high and low temperatures, by similar increments of temperature. The force of carbonic acid was found to be equal to that of air compressed to $\frac{1}{20}$ at 12°, and of air compressed, to $\frac{1}{30}$ at 32°, making an increase equal to the weight of thirteen atmospheres, by an increase of 20° of temperature.

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* See Quarterly Journal, vol. v. p. 74.

### 7. *On the Temperature at considerable depths of the Caribbean Sea.*

*By* Captain Edward Sabine, F.R.S.

[In a Letter addressed to Sir H. DAVY, Bart., P.R.S.]

Captain Sabine found the temperature of the water, at a depth of 6000 feet, in latitude $20\frac{1}{2}$ N. and long. $83\frac{1}{2}$ W. near the junction of the Mexican and Caribbean Seas, to be 45°.5, that of the surface being 83°. He infers, that one or two hundred fathoms more line, would have caused the thermometer to descend into water at its maximum of density as depends on heat; this inference being on the presumption that the greatest density of salt water occurs, as is the case in fresh water, at several degrees above its freezing point.

### 8. *Letter from* Captain Basil Hall, R.N., *to* Captain Kater, *communicating the details of Experiments made by him and* Mr. Henry Foster, *with an Invariable Pendulum; in London; at the Galapagos Islands in the Pacific Ocean, near the Equator; at San Blas de California on the N.W. Coast of Mexico; and at Rio de Janeiro in Brazil. With an Appendix, containing the Second Series of Experiments in London, on the Return.*

The title is an abstract of the paper, and the following are the most exact results obtained by Captain Hall at each station.

| Stations. | Diminution of Gravity From Pole to Equator. | Ellipticity. | Length of Equat. Pend. |
|---|---|---|---|
| Galapagos, 0° 32′ ″ N | .0051412 | $\frac{1}{284.98}$ | 39.017196 |
| San Blas, 21 30 25 N | .0054611 | $\frac{1}{313.55}$ | 39.00904 |
| Rio, . . . 22 55 22 S | .0053431 | $\frac{1}{302.37}$ | 39.01206 |

### 9. *Second Part of the Paper on the Nerves of the Orbit. By* Chas. Bell, Esq.

[Communicated by Sir H. Davy, Bart., P.R.S.]

This is a continuation of the subject upon which Mr. Bell had entered in his last paper. His object is to explain the reason of there being six nerves distributed to the eye, and crowded into the narrow space of the orbit. In this investigation he demonstrates, that there is a correspondence between the compound functions of an organ and the nerves transmitted to it. It is impossible to do more, than here sum up the distinct functions of the nerves, as unravelled by the skill of the author.

" The first nerve is provided with a sensibility to effluvia, and is properly called the olfactory nerve.

" The second is the optic nerve, and all impressions upon it excite only sensations of light.

" The third nerve goes to the muscles of the eye solely, and is a voluntary nerve by which the eye is directed to objects.

" The fourth nerve performs the insensible traversing motions of the eyeball. It combines the motions of the eyeball and eyelids, and connects the eye with the respiratory system.

" The fifth is the universal nerve of sensation to the head and face, to the skin, to the surfaces of the eye, the cavities of the nose, the mouth and tongue.

" The sixth nerve is a muscular and voluntary nerve of the eye.

" The seventh is the auditory nerve, and the division of it, called *portio dura*, is the motor nerve of the face and eyelids, and the respiratory nerve, and that on which the expression of the face depends.

" The eighth, and the accessory nerve, are respiratory nerves.

" The ninth nerve is the motor of the tongue.

" The tenth is the first of the spinal nerves; it has a double root and a double office; it is both a muscular and a sensitive nerve."

Mr. Bell concludes his paper with a few very appropriate words in favour of anatomy, as a means better adapted for discovery than experiment.

" Anatomy," he observes, " is already looked upon with prejudice by the thoughtless and ignorant: let not its professors unnecessarily incur the censures of the humane. Experiments have never been the means of discovery; and a survey of what has been attempted of late years in physiology will prove, that the opening of living animals has done more to perpetuate error, than to confirm the just views taken from the study of anatomy and natural motion."

With another opinion of Mr. Bell's we cannot also but coincide, and that is, that " Medical histories do not often lead to the improvement of strict science."

It is an opinion worthy the consideration of the Committee of Papers of the Royal Society.

### 10. *An Account of Experiments made with an Invariable Pendulum at New South Wales. By* Major-General Sir Thomas Brisbane, K.C.B. F.R.S.

[Communicated by Captain HENRY KATER, F.R.S., in a Letter addressed to Sir H. DAVY, Bart., P.R.S.]

The results of Sir Thomas Brisbane's experiments are as follow: 39·07696 inches the length of the pendulum, vibrating seconds at Paramatta; .0052704 the diminution of gravity from the pole to the equator, and $\frac{1}{295,84}$ the resulting compression.

### 11. *Observations and Experiments on the daily Variation of the Horizontal and Dipping Needles under a reduced directive Power. By* Peter Barlow, Esq., F.R.S., of the Royal Military Academy.

[Communicated by DAVIES GILBERT, Esq., V.P.R.S.]

The daily change of the horizontal needle is so small, that it has only hitherto been detected with the most careful observations, and with the most delicate instruments; and in the dipping needle, that change is so extremely minute, as to have escaped observation altogether. It occurred to Mr. Barlow, that it would be possible to increase this deviation in both needles, so as to render it distinctly observable, by reducing the directive power of the needle, by means of one or two magnets properly disposed, to mask, at least in part, the terrestrial influence. This idea was realized, and in this way it is easy to produce a daily variation, to almost any amount. From his experiments, Mr. Barlow draws the following conclusions:—

1st. That while the north end of the needle is directed to any point from the south to N.N.W. its motion during the forenoon is towards the left hand, advancing to some point between the N.N.W. and north; and while it is directed towards any point between the north and S.S.E., it passes to the right hand, that is, still to some point between the north and N.N.W.

2dly. That the daily change is not produced by a general deflection of the directive power of the earth, but by an increase and decrease of attraction, of some point situated between the north and N.N.W., or between the south and S.S.E.

3dly. That the dipping needle is subject to a daily variation, which cannot, at present, be reduced to any fixed principles.

### 12. *On the Diurnal Deviations of the Horizontal Needle when under the influence of Magnets. By* Samuel Hunter Christie, Esq., M.A., Fellow of the Cambridge Philosophical Society: of the Royal Military Academy.

[Communicated by Sir H. DAVY, Bart., P.R.S.]

Mr. Barlow communicated to Mr. Christie his method of rendering the variations of the magnetic needles more sensible, and he commenced a series of observations in consequence of the communication.

He ascertained, that there was an easterly deviation before eight o'clock in the morning, and that the greatest westerly deviation took place about one o'clock in the afternoon. He also found, that the state of the weather had a considerable influence upon the nature and extent of the changes. But the most striking effects seemed to him to arise from changes of temperature, and he adopts the opinion that temperature, if not the only cause of the daily variation, is the principal. He expresses his intention of entering fully into the general question, when he shall have ascertained the precise effects of changes in the temperature of magnets.

### 13. *On Fossil Shells.* *By* Lewis Weston Dillwyn, Esq., F.R.S.

**[In a Letter addressed to Sir H. Davy, Bart., P.R.S.]**

Mr. Dillwyn remarks, that every turbinated univalve of the older beds, from transition lime to the lias, of which he can find any record, belongs to the herbivorous genera, and that the family has been handed down through all the successive strata, and still inhabits our land and waters. On the other hand, all the carnivoruos genera abound in the strata above the chalk, but are comparatively extremely rare in the secondary strata, and not a single shell has been detected in any lower bed than the lower oolite. He thinks, that a further examination will prove, that neither the aporrhaides or any of those few undoubtedly carnivorous species, which have been found in the secondary formations, were furnished with predaceous powers, but that they belong to a subdivision of the trachelipoda zoophaga, which feed only on dead animals.

### 14. *On the apparent Magnetism of Metallic Titanium.* *By* William Hyde Wollaston, M.D., V.P.R.S.

In this paper Dr. Wollaston corrects an oversight in his former communication upon metallic titanium. He therein stated, that when the crystals from the slag had been freed from all particles of iron adherent to them, they appeared to be no longer acted upon by the magnet. He has since found, that although they are not sufficiently attractive to be wholly supported by the magnet, yet, when a crystal is supported by a thread, the force of attraction is sufficient to draw it twenty degrees from the perpendicular. From an ingenious comparison of different magnetic forces, he calculates that $\frac{1}{250}$ part of iron, as an alloy in the metallic titanium, would be sufficient to account for this power; and he shews, that it is extremely difficult chemically to detect so minute a portion of iron, on account of the high colour of the precipitates of titanium.

15. *An Account of the Effect of Mercurial Vapours on the Crew of His Majesty's Ship Triumph, in the year* 1810. *By* William Burnett, M.D., *one of the Medical Commissioners of the Navy, formerly Physician and Inspector of Hospitals to the Mediterranean Fleet. Communicated by* Matthew Baillie, M.D., F.R.S.

The particulars of this curious case have been already published by Dr. Baird, in *Nicholson's Journal*, for the month of Oct. 1810.

16. *On the Astronomical Refractions. By* J. Ivory, A.M., F.R.S.

This is a very long and laborious investigation of the problem of astronomical refraction; its result is a new table of refractions with which the paper concludes, and which is compared with other tables that have been long in the hands of astronomers, and the characters of which are well established. Mr. Ivory shews that it is fruitless to expect a near agreement in every single instance between observation and any table of refractions whatever, and that there is no test of their accuracy except the smallness of the mean error in a series of observations made at different times.

17. *Observations on Air found in the Pleura, in a case of Pneumatothorax; with Experiments on the Absorption of different kinds of air introduced into the pleura. By* John Davy, M.D., F.R.S.

This is a medical history which Dr. Davy has endeavoured to illustrate by some experiments upon dogs. He observes that the circumstances which he has ventured to bring forward are somewhat favourable to the idea of the secretion or exhalation of azote, but are still far from conclusive.

18. *On Bitumen in Stones. By* the Right Honourable George Knox, F.R.S.

This is a second paper upon the same subject. Mr. Knox finds bitumen in every thing except rock crystal and pearl-white adularia.

19. *On certain Changes which appear to have taken place in the Positions of some of the principal fixed Stars. By* John Pond, Astronomer Royal, F.R.S.

The Astronomer Royal thinks that his observations lead to the conclusion that some variation, either continued or periodical, takes place in the sidereal system, which producing but very small deviations in a finite portion of time, has hitherto escaped notice. The nature of this motion appears to be such that the stars are now mostly found a considerable quantity to the southward of their computed planes. With respect to the laws by which these motions are governed, the observations in question, he admits, are not sufficiently exact to throw any light upon them.

[To the Editor of the QUARTERLY JOURNAL OF SCIENCE, *&c.*]

*Manchester, March* 1, 1824.

SIR,

THE review of the 9th edition of my Elements of Chemistry, in the last number of your Journal, contains some animadversions, to which I trust you will do me the justice to insert a brief reply. It is not, indeed, my intention to follow the reviewer through the variety of topics which he has introduced, but to confine myself to a few of those, on which I am most desirous to be set right with your readers, and which involve questions of some importance to chemical philosophy.

It has happened unfortunately that a passage, expressing doubts of the correctness of the theory of volumes, which certainly ought to have been expunged from the present edition of my work, was overlooked, owing to one or two of the early sheets having been revised under circumstances disadvantageous to correctness. For this oversight, I am content to take upon myself whatever blame it may justly deserve; and I should have had no reason to complain, had the reviewer pointed out the striking inconsistency of the passage, which he has quoted, with other parts of my volumes. At page 299, vol. i., for example, I state, "analogy is certainly in favour of this opinion, for the instances are numerous in which gaseous bodies observe the law respecting volumes deduced by Gay-Lussac, and we have not at present any well-ascertained exception to it." The tenor of the whole work, also, is inconsistent with the rejection of the theory of volumes imputed to me by the reviewer; for almost every chapter affords examples of compounds constituted in conformity to the law; and at the close of the second volume I have inserted, for the first time, a table exhibiting a general view of such compounds.

The reviewer complains (p. 338,) that I have not given a more elaborate and consistent account of the atomic theory, though he represents it (p. 339) as requiring "mystifications," and particularly marks the distinction between the atomic *hypothesis* and the *theory* of volumes. To a certain extent, the law of volumes is, I admit, the expression of a general fact, of which we have the indubitable testimony of our senses. But with regard to certain elementary substances, which are not known to us separately in a gaseous state, it is entirely matter of inference that *their* vapours unite in volumes, which are either equal, or multiples or sub-multiples of each other. We have, for example, no argument but from analogy, that this holds with respect to carbon; nor, if we admit the probability of such combinations, have we any decisive proof that the volumes, which have been assigned, are actually the true ones. In all such cases, where we have not access to the facts by direct experiment, the law of volumes rests on the ground of analogy only; and is so far purely theoretical. The law, also, however, well established with respect to gaseous bodies, is limited to them only; and we must seek for some other principle, to explain the far greater number of chemical combinations which take place between bodies existing under other forms.

In the investigations which have led Mr. Dalton to the atomic system, it appears to me that he has pursued no other method of reasoning than that which has been followed by the most successful cultivators of natural science, since the introduction of the inductive logic. The theory of gravitation itself, however firmly it may now be established, took its rise in an hypothesis founded on analogy, and could be considered as nothing more than an hypothesis, till that period of the life of its great author, when the coincidence was ascertained between the law which regulates the fall of heavy bodies, and that power which preserves the moon in her orbit. "A principle," it has been remarked by the late Professor Playfair, "is often admitted in physics, merely because it explains a great number of appearances, and the theory of gravitation itself rests on no other foundation*." The term hypothesis, then, is far from being one of just reproach, since it may be applied in a variety of cases to those first steps which it has been found necessary to take in philosophical inquiries, and which have led eventually to well-established laws.

The views of Mr. Dalton respecting the atomic constitution of bodies appear to me to be founded mainly on the general fact, *that bodies unite in definite proportions*. Of this general truth, Richter certainly furnished the best and fullest evidences. Far from wishing to "suppress" the share of credit to which he is entitled, I have alluded to the table, calculated by Fischer from his experiments; but it is omitted in the appendix to the present edition, merely because it has been superseded by the more extensive tables of equivalents, which have since been constructed. The law of combination *in multiple proportions*, the first experimental proofs of which are due to Mr. Dalton, comes strongly in aid of the atomic theory, and furnishes its most striking proofs and illustrations. Nothing can be more evident than that if we set out from a binary compound, whose gaseous elements exist in equal volumes, and proceed to compounds of the same elements, in which either is found as a multiple in volume of the other, there must, as the reviewer observes, "be a perfect accordance between the atomic hypothesis and the theory of volumes." But the atomic theory is, I contend, a wider and more comprehensive generalization, and includes the law of volumes as well as that of combination by multiples of weights. In this case, as in many others, when we advance from discovery to discovery, we do nothing more than resolve our former conclusions into others still more general.

There can surely be nothing inconsistent with sound philosophy in inquiring *why* bodies unite in definite proportions, and *why* they unite in proportions which are multiples or sub-multiples of weights or of volumes; and the only satisfactory explanation, that has yet been given of these facts is, that in those combining weights, which are represented by equivalent numbers, are contained determinate numbers of ultimate particles or atoms, and that from the relative weights of aggregates that combine, we may deduce the proportions as to weight which the ultimate single atoms bear to each other. As there seems every reason to believe that chemical attraction is exerted, not between masses, but between ultimate particles or atoms only, combination will then take place either between single atoms or when either is in excess, the excess will be represented by some simple multiple of the number of atoms. In this reasoning it is of course taken for granted that matter is not infinitely divisible, a position rendered extremely probable by a philosopher, to whose opinions the reviewer will agree with me in paying the greatest deference. "Now though we have not the means," that writer observes, "of ascertaining the extent of our own atmosphere, those of other planetary bodies are nevertheless objects for astronomical investigation; and it may be deserving of consideration, whether, in any instance, a deficiency of such matter can be proved, and whether, from this source, any conclusive argument can be drawn in favour of ultimate atoms of matter in general. For since the law of definite proportions, discovered by chemists, is the same for all kinds of matter, whether solid, fluid, or elastic, if it can be ascertained that any one body consists of particles no longer divisible, we can then scarcely doubt that all other bodies are similarly constituted; and we may, without hesitation, conclude that those equivalent quantities, which we have learned to appreciate by proportionate numbers, do really express the relative weights of elementary atoms, the ultimate objects of chemical research*." A body so constituted (it is the scope of the essay which has been just quoted to shew) is found in the earth's atmosphere, all the phenomema according with the supposition that it is "of finite extent, limited by the weight of ultimate atoms of definite magnitude, no longer divisible by repulsion of their parts."

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* Playfair's Works, vol. iv. p. 62, note.

But though the atomic theory, in its general outline, seems to me to rest sufficiently on the evidence of facts, and on legitimate reasoning, yet there are some positions which have arisen out of it, that may or may not be true, without, in the latter case, impeaching its general correctness. Of this nature are the two cited by the reviewer, (p. 340) especially the first, *viz.*, "that an increase of the density of a gas indicates an increased number of simple atoms associated in the compound atom." This principle, I am ready to admit, may have been too hastily deduced; for besides that it is at variance with the view which I have adopted of the nitrous compounds, it is inconsistent also with that which I have taken of the compounds of carbon and hydrogen; olefiant gas, the binary compound, being denser than light carburetted hydrogen, the ternary one. The other position, that "of chemical compounds the most simple, is the most difficult to be decomposed," stands unimpeached, and is exemplified, as the reviewer himself remarks, in the greater difficulty of decomposing nitrous oxide than nitrous gas. To Mr. Dalton's opinion of nitrous gas, which makes it the binary compound, its greater facility of decomposition might present a reasonable objection. But it is quite inconsistent with sound reasoning to frame a proposition out of Mr. Dalton's views and mine, which are completely at variance as to the compounds of nitrogen, and to apply to that proposition the syllogistic method of reasoning as a test of its truth. No syllogism can be so constructed as to involve in the same dilemma two persons, who disagree with each other as to the conditional proposition on which that syllogism is founded.

Though I have adopted, as most probable, that view of the nitrous compounds which makes the elements of nitrous oxide to exist in binary and those of nitrous gas in ternary, atomic proportion, yetI consider the truth of this opinion as far from being demonstrated That the volumes of the elements of those two compounds are what they have been represented by Gay-Lussac, I entertain very little doubt, not only from the evidence of other persons, but from methods of analysis which I have myself devised, and which, though not otherwise important, than as they bring out the results by easy and summary processes, I shall probably ere long lay before the public. But it must still remain a subject of inquiry, whether equal volumes of nitrogen and oxygen gases contain, as Mr. Dalton supposes, equal numbers of atoms; or whether, as I take to be more probable, the same number of atoms exists in one volume of oxygen as in two of nitrogen gas.

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* Dr. Wollaston on the Finite Extent of the Atmosphere.—*Phil. Trans.* 1822.

The illustrious author of the *Elements of Chemical Philosophy* will not, I trust, require any assurance from me, that nothing could be farther from my design, or more repugnant to my feelings, than to misunderstand "intentionally" his ideas respecting chemical combination. I have, it is true, rendered the word *proportion* by that of *atom*, but I have enclosed the latter word in parentheses, purposely to shew that it was not the expression of the author, but my own interpretation of his meaning. The fact is, that great ambiguity has arisen out of the use which has been made of the word *proportion*. Strictly, the only numerical expressions of proportion, that can be considered as "the results of experiment," must be derived from a comparison either of the weights, or of the volumes, in which bodies unite; and it appears to me that a system of numbers, derived from the consideration of weights, should be kept distinct from one derived from a comparison of volumes. But the numbers (1 and 15) representing hydrogen and oxygen, were gained from the joint consideration of the *weight and volume* of the elements of water; while those representing oxygen and nitrogen (15 and 26) were derived from a comparison of the *weights only* of the elements of nitrous oxide. Since, then, the word proportion could not, in both cases, apply to a comparison of weights only, nor yet of volumes only, it was natural for me to conclude that it must bear a reference to ultimate particles or atoms, the only other objects, which I could conceive as, in this case, admitting of being compared.

These, Sir, are the only points respecting which I deem it necessary to trespass on the attention of your readers, though there are others on which I am not disposed to concede the justice of the reviewer's strictures. In some instances, I allow, he has pointed out mistakes that may call for correction on a future occasion, should any occur to me. Having invited the communication of errors or omissions, with a view to the improvement of my volumes, it would ill become me to feel "offended" when that invitation is complied with; and all that I claim is to be animadverted upon with a reasonable share of courtesy and of candour.

I am, Sir,

Your obedient and faithful servant,

WILLIAM HENRY.

# Art. XIII. PROGRESS OF FOREIGN SCIENCE.

In Volume XIII. p. 144, we briefly animadverted on some researches of Professor Gmelin, of Tübingen, published in Dr. Brewster's Journal, about two years ago, where the terms refute, and refutation were applied with more freedom, than propriety or decorum, to Sir. H. Davy's fundamental experiments relative to the connexion of chemical affinity with electrical attractions, contained in his Bakerian Lecture of 1806. M. Becquerel read to the Academy of Sciences on the 7th June, 1823, an interesting paper on the electrical effects which are developed during different chemical actions, which perfectly accord with, and seem fully to confirm, the conclusions of the English philosopher. After a candid retrospect of preceding inquiries on the subject, M. Becquerel thus states Sir H. Davy's theory: " Supposing two bodies, whose molecules are in different states of electricity, and that these states are sufficiently exalted to give them an attractive force, superior to the power of aggregation, a combination will be formed. This is the key of the electro-chemical theory." " Although Sir H. Davy has advanced the opinion, that the substances which combine are those which manifest on mutual contact, opposite electrical states, yet we perceive from his own experiments that it is by induction he extended this property to all the bodies which exert chemical actions on one another; for instance, he was not able to verify it on alkaline and acid substances, unless they were perfectly dry. In other cases, the results were null. He adduces, among others, pure potash, and sulphuric acid, which afford no appearance of electricity at the moment of their combination. In fact, this celebrated chemist could not recognise electricity in the contact of two substances which are just combining; for, adopting the electro-chemical theory, as soon as the combination takes place, the two electricities that were developed, recombine, and probably form, by their union, caloric: whence, in making use of a condenser to collect one of the electricities which is disengaged, traces of this fluid ought to be found with difficulty, since the condenser requires a certain time to charge itself, during which the two electricities may re-combine. But if a galvanometrical multiplier be employed, such as that of M. Schweigger, which renders the electricities sensible at the very instant of their disengagement, and consequently at the instant when the combination takes place, currents will be obtained of greater or less force, according to the degree of conductibility of the substances put in action, and that of their reciprocal affinities; I say according to the degree of conductibility, because when one of these substances conducts the electricity ill, there is no current, although the chemical action be very strong. The conductibility then is here an indispensable condition.

We shall examine in succession the electrical effects that we have observed in different chemical actions by the aid of the multiplier; *viz.*—1. At the moment of the combination of acids with metals and alkalis. 2. In the dissolutions. 3. In the contact of metallic oxides with the alkalis which combine with them. 4. In the precipitates. As to double decompositions, it has been impossible for me to recognise the slightest trace of electricity at the moment of their formation.

### *Electrical Effects produced at the moment of the Combination of the Metals and Alkalis with the Acids.*

We have seen above that Sir H. Davy observed electrical effects on the contact of acids and alkalis, only when these bodies had been perfectly dried. M. Œrsted asserts that he has perceived them at the instant when the acid combines with the metal.

The following is the means which I employ to shew the electrical effects in these species of actions. I make use of a galvanometer, whose wire is of platinum. (See p. 124 of this volume.) At one of the extremities of this wire I placed a little platinum spoon destined to receive the acid, which is selected of such a nature as not to act on the platinum. To the other end of the wire is adapted a piece of the same metal, between the branches of which (as pincers) the body is placed, which is to act on the acid. In case the platinum could exert an electro-motive action on this body, there is placed between them a bit of moistened paper. Let us begin by shewing what electrical effects result at different temperatures from the contact of a liquid with the platinum. At the ordinary temperature, whatever be the liquid, provided it is not nitro-muriatic acid, the electrical current is null, but when the temperature is raised, phenomena occur which we shall endeavour to explain. Let us put into the spoon distilled water, and let us raise the temperature to ebullition, there will be no current in consequence; if the water of the Seine be used, the current will be extremely feeble, and it will increase in intensity by the addition of a little nitric acid, or alkali. Now, since we know that boiling nitric acid has no more action on platinum than cold nitric acid, it is hence probable that the current is owing to the difference of temperature of the two ends of the wire. It has been already shewn in a former memoir that two pieces of the same metal, in a sufficiently unequal state of temperature, pass, on their mutual contact, into two different electrical states. This change of temperature must therefore be avoided, which is done by using small fragments of the bodies to be acted on, and a large platinum spoon.

Let us now fix in the platinum forceps a little bit of caustic, soda, or potash, slightly moistened with water. At the moment when the alkali touches the acid, an energetic electrical current will take place, which will proceed from the acid to the alkali following the curcuit. Thus at the instant of contact of these two bodies the acid becomes enveloped with an atmosphere of positive electricity, and the alkali with one of negative. The electrical current is so strong that it may be observed without a galvanometer. It is sufficient for this purpose to present the conjunctive wire to a needle suspended at the filament of a silk-worm. In order to observe the electrical currents which result from the action of an acid on a metal, the same process is employed; care only is taken to prevent the metal touching the platinum directly, by interposing a small slip of paper. The experiment is made in the same way, and the result is the same, whatever be the acid and the base. M. Becquerel next shews that during the solution of a body in water, or alcohol, no electricity is produced. But the smallest acid or alkaline particles are sufficient to modify the results.

He then details some experiments on the solution in caustic potash of metallic oxides, such as oxide of zinc, and of lead newly precipitated. In these, electrical phenomena were exhibited. Whenever the oxide (generally contained in the thin cæcum of an animal,) touches the alkaline solution, the needle deviates from its magnetic direction, and the current goes from the oxide to the alkali, passing along the wire. Hence in these kinds of combinations, the oxides comport themselves like acids, and the alkalis are always surrounded with an atmosphere of negative electricity, as in their actions on the acids.

In slow precipitations, as when an infusion of nut galls acts on sulphate of iron, a current is developed which goes from the infusion to the sulphate. Let us put a solution of sulphate of magnesia in contact with the caustic potash contained in the membranous bag. The needle will deviate slightly from its direction, and the current will be from the sulphate to the alkali. In making nitrate of barytes act on sulphuric acid, the current goes from the acid to the nitrate. When two perfectly neutral salts were employed, as sulphate of soda, and nitrate of barytes, he has not been able to discern the least appearance of a current.

In a subjoined notice, M. Becquerel describes the following experiment. Take a plate of platinum, and placing it horizontally, fix by cement, two glass tubes vertically upon it. Liquids poured into these tubes will communicate through the medium of the platinum plate. Let us pour in any liquids whatever; if they are susceptible of exerting chemical actions on the two ends of the wire of the galvanometer which are immersed, there will be naturally established an electrical current, since the plate of platinum permits the electricity to circulate from one liquid to the other. Suppose one of these liquids to be concentrated, and the other dilute nitric acid. On plunging into each tube an end of the copper wire of the galvanometer, the experiment will shew that the electrical current goes from the stronger acid to the other. Let us now substitute, in the place of one of these acids, ammonia, which dissolves the oxide of copper. At the instant of immersion of the two wires the current will go from the acid to the alkali, and will continue to move in the same direction, even when the acid shall be diluted with water.—*Ann. de Chem. et de Phys.* xxiii. p. 244.

### HEAT. *On the Property which some Metals possess of facilitating the Combination of Elastic Fluids. By* MM. Dulong *and* Thenard.

After exhibiting to the Academy of Sciences Dobereiner's interesting experiment described in our last number, this gentleman proceeded to detail some modifications of it which they had devised. On immersing some spongy platinum into a mixture of two parts of hydrogen and one of oxygen, explosion takes place. If much azote be present, the water is slowly and actually formed. The sponge of platinum, when strongly calcined, loses the property of becoming incandescent; but in this state it produces slowly, and without any very sensible elevation of temperature, the combination of the gases. Platinum reduced into a very fine powder by a well-known chemical process, has no action whatever at the ordinary temperature. The same is the result with wires or laminæ. It might thence be supposed that the porosity of the metal was an essential condition of the phenomenon, but the following facts destroy this conjecture. Platinum was reduced into leaves, as thin as the malleability of this metal allows. In this state the platinum acts at the ordinary temperature, on the mixture of oxygen and hydrogen, with the greater rapidity the thiner its leaf is. They procured some which caused detonation after some instants. But what renders this action still more extraordinary is the physical condition indispensable for its development. A very thin leaf of platinum, rolled round a cylinder of glass, or suspended freely in an explosive mixture, produced no sensible effect at the end of several days. The same leaf crushed together like the wadding of a musket, acts instantaneously, making the mixture explode. Rolled leaves and wires at temperatures of from 200° to 500° cent. act slowly, but without explosion.

Thin leaves of gold and silver act only at elevated temperatures; but always below that of boiling mercury. Silver is less efficacious than gold. In accordance with Sir H. Davy's results with palladium and platinum in the safety-lamp these gentlemen found, that these two metals, when of the same thickness, acted equally well.

The oxide of carbon and oxygen combine, and nitrous gas is decomposed by hydrogen at the ordinary temperature, when they are in contact of the sponge of platinum. Olefiant gas mixed with a proper quantity of oxygen, is completely transformed into water and carbonic acid by the sponge of platinum, but only at a temperature above 300° cent.

M. Thenard long ago shewed that iron, copper, gold, silver, and platinum, had the property of decomposing ammonia at a certain temperature, without absolving any of the principles of this alkali; and that this property seemed to be inexhaustible. Iron possesses it in a higher degree than copper, and copper more than silver; gold and platinum under equal surfaces. Ten grammes of iron wire are sufficient to decompose, within a few hundred parts, a current of ammoniacal gas pretty rapid, and kept up for eight or ten hours, without the temperature exceeding the point at which ammonia completely resists decomposition. A tripple quantity of platinum wire, of the same size, does not produce a nearly similar effect, even at a higher temperature.

Palladium, in a spongy mass, inflames a stream of hydrogen, as well as platinum. Iridium under this form becomes very hot, with the production of water; nickel and cobalt, in mass, determine at about 300° cent. the union of hydrogen and oxygen; lastly, the sponge of platinum forms, in the cold, water and ammonia, with nitrous gas and hydrogen, and acts also on a mixture of hydrogen and protoxide of azote. M. Gay-Lussac's hydrogen lamp answers well for the experiment of ascension, as the hydrogen would issue in a very small stream. By holding a very light bit of platinum sponge, about three quarters of an inch, before the orifice, the effluent gas is instantly kindled. This is more convenient than the trophorus plate.—*Ann. de Chim. et de Phys.* xxiii. 440.

## *On the Preparation of Oxide of Uranium. By* MM. Lecanu *and* Serbat.

The authors of this process, after having fused the pulverized mineral (pech-blende,) with one half of its weight of nitre, washed the mass which results from the operation, treated the residiuum with nitric acid, evaporated the solution to dryness, and re-dissolved in water acidulated with the same acid, add to the solution an excess of carbonate of ammonia, which, while it is sufficient to re-dissolve the whole oxide of uranium, has no action on the carbonates of lead and lime. M. Laugier, in commenting on the above process, recommends the use of one part and a half of nitre, instead of half a part. The solution containing the nitrate of ammonia, and the carbonate of uranium is to be evaporated to dryness and calcined, in order to get the pure oxide. M. Laugier advises in preference to wash away with hot water the nitrate of ammonia, and to calcine the remaining carbonate of uranium which has in the filter, a fine lemon yellow colour.—*Journal de Pharmacie*, March, 1823.

### *On the Oxides of Nickel. By* M. J. P. Lassaigne.

The metal was purified by Laugier's process. The protoxide is obtained from solutions in acids, it is of an ash-grey colour, gives green solutions with acids, from which caustic alkalis precipitate it of apple-green colour. Its constituents are

| | |
|---|---|
| Nickel . . . . . . . . . . . . . . | 100 |
| Oxygen . . . . . . . . . . . . . . | 20 |

Whence the atomic weight of nickel appears to be 5. The deutoxide is of a brilliant black colour, having some analogy with the peroxide of manganese. At a red heat it gives up a portion of its oxygen, and passes to the state of protoxide. It is prepared by treating the hydrated protoxide with chlorine. M. Lassaigne's experiments on its composition, give

| | |
|---|---|
| Nickel . . . . . . . . . . . . . . | 100 |
| Oxygen . . . . . . . . . . . . . . | 39·44, |

approaching sufficiently near to 40.

The sulphuret artificially made is of a brilliant yellow colour, like iron pyrites, and is very brittle. It consists of

| | |
|---|---|
| Nickel . . . . . . . . . . . . . . | 100 |
| Sulphur . . . . . . . . . . . . . . | 40 |

or one atom of each.

He describes a chloride and bichloride, and an iodide, whose constitution may be inferred from the above numbers.

### *On the Capacity of Saturation of Delphia. By* M. Feneulle, *of Cambray.*

| | | | |
|---|---|---|---|
| Neutral Sulphate—Acid . . . | 3·031 | 5·0 |
| Delphia . . | 96·969 | 16·0 |

Subsulphate—not distinctly characterized. It seems to have a double dose of base. The muriate of delphia is amorphous like the preceding. It is formed of

| | | |
|---|---|---|
| Muriatic Acid . . . | 100 | 2·136 |
| Delphia . . . . . | 4675 | 100·000 |

There is also a submuriate. It consists of

| | |
|---|---|
| Acid . . . . | 1·194 |
| Delphia . . . | 100·000 |

## *Facts subservient to the History of the Succinic and Benzoic Acids.*

### *By* MM. Lecanu *and* Serbat.

Subjected to the action of heat these acids comport themselves in nearly the same way. They melt, then are volatilized, leaving always a slight carbonaceous residuum. The difference of solubility of these acids in water, as well as in the essential oil of turpentine, establishes a remarkable difference between them. While, in fact, at the temperature of 16° cent., water dissolves scarcely an appreciable quantity of benzoic acid, and, at 100°, only one-twelfth part of its own weight; 100 parts of water, at 16°, dissolve 20 parts. and at 100°, about 46 parts of succinic acid. On the other hand, at the temperature of 16° cent., a gramme of benzoic acid requires for solution only 249 parts of essential oil of turpentine, and at 100° much less than its weight. Hence the liquor, on cooling, concretes into a mass. Succinic acid, even above 100°, dissolves in it very sparingly, although the essence thereby acquires the property of reddening litmus pretty strongly. Hitherto the property of separating iron from manganese, forming with the first an insoluble salt, and with the second a soluble one, seemed to belong only to the benzoic and succinic acids. It is, however, met with in the camphoric and pyrolartaric acids. The last even, would appear even to be capable of separating these metals more completely than succinic acid does. Perhaps it would be advantageous, in regard to economy, to substitute it for this acid.

Succinic acid is not altered by being distilled into nitric acid, diluted with its own weight of water. It is, therefore, not convertible, like some of the other vegetable acids, into the oxalic. Nitric acid becomes thus the most convenient agent for purifying the succinic. The action of nitric acid on the benzoic has not been well investigated, even by these gentlemen; but it is not transformed into the oxalic acid. Succinic acid affords with potash a very deliquescent salt; with soda, a salt unchangeable in the air, or rather somewhat efflorescent, and crystallizing in plates like nitrate of silver; with ammonia, a slightly deliquescent salt, very soluble in water, and crystallizing in long prisms with four faces, transparent and colourless. It occurs frequently in plates; with barytes, a salt hardly soluble, which is obtained in the form of a white powder, by evaporating its solution. They were prevented by an accident from examining the salt that they had obtained with benzoic acid.

These two acids precipitate copper, tin, silver; these precipitates, insoluble in water, are re-dissolved with facility by acetate of potash, and nitrate of soda, without the nitrate of potash, the sulphate and muriate of soda appearing to possess the same property.—*Journal de Pharmacie, for February,* 1823.

## *Memoir on the Milk of the Cow Tree (Palo de Vaca). By* J. B. Boussingault *and* Mariano de Rivero.

Among the astonishing vegetable productions that are met with at every step in the equinoctial regions, a tree is found which yields in abundance a milky juice comparable in its properties to the milk of animals, and which is employed for the same purposes, as M. de Humboldt witnessed at the farm of Barbula (Cordillere littorale de Venezuela), where he drank some of the milky juice. The tree grows in considerable numbers on the mountains which command Periquito, situated to the north-west of Maracay, a village to the west of the Caraccas. The vegetable milk possesses the same physical properties as that of the cow, with the single difference, that it is a little viscid. It has the same taste. In its chemical properties, it differs sensibly from animal milk.

It mixes with water in all proportions, and when thus diluted, it does not coagulate by ebullition. The acids do not convert it into clots, as happens to cow's milk. Ammonia, instead of causing a precipitate, renders it more liquid. This character indicates the absence of caoutchouc. Alcohol occasions a feeble coagulation, or rather renders the juice more easy of filtration. The recent juice slightly reddens litmus. Its boiling temperature is the same as that of water. Exposed to heat, it exhibits at first the same phenomena as cow's milk. A pellicle is formed at its surface, which prevents the disengagement of aqueous vapours. On removing the successive pellicles, and evaporating it at a gentle heat, an extract is obtained resembling frangipane; when the action of heat is longer continued, oily drops are formed, which increase according as the water is carried off, and finally afford an oily liquid, in which a fibrous matter floats which becomes dry and horny, as the temperature of the oil is raised. Then is diffused the best characteristic odour of meat frying in grease. By the action of heat, therefore, the vegetable milk is separable into two parts, the one fusible and of a fat nature, the other fibrous and of an animal nature. If the evaporation of the vegetable milk is not pushed too far, and if the fusible matter be not raised to ebullition, it may be obtained without alteration. It then possesses the following properties:—

It is of a white slightly yellowish colour, translucid, solid, and resists the impression of the finger. It begins to melt at 40° centig., and when the fusion is completed, the thermometer indicates 60°. Alcohol of 40° (sp. gr. 0·817) dissolves it totally by ebullition, and it precipitates on cooling. It saponifies with caustic potash, and with ammonia forms a soapy emulsion. Nitric acid heated on it, dissolves and converts it into oxalic acid. It resembles refined bees' wax, and serves for making candles. The fibrous substance is procured by decanting the melted waxy matter, washing off the last portions of it with an essential oil, squeezing the residuum, and boiling it a long time in water, to volatilize the oil, the odour of which cannot, however, be thereby completely discharged. Thus obtained, the fibrous matter is brown, having been somewhat altered by the temperature of the melted wax. It is tasteless. Placed on a hot iron, it twists itself and swells up, melts and is carbonized, diffusing the smell of broiled meat. Alcohol does not dissolve it; and hence by treating the extract of the vegetable milk repeatedly with hot alcohol, the fibrous matter is obtained white and flexible. In this state, it dissolves readily in diluted muriatic acid. It possesses the same properties, therefore, as animal fibrine. Fibrine had already been found in the milky juice of the Carica papaya, by Vauquelin. Besides these two main constituents, the vegetable milk contains a little sugar, a magnesian salt (not an acetate), and water. It contains neither caseum nor caoutchouc. By incineration, some silica, lime, phosphate of lime, and magnesia were obtained. The wax forms about one-half the weight of the milk.—*Ann. de Chim. et de Phys.* xxiii. 219.

### *On the Hot Mineral Waters of the Cordilleras of Venezuela. By* the same.

The springs of Onoto issue copiously from gneiss. Their temperature is 44°·5 centig. Their height above the level of the sea, is 702 metres. From the bottom of each reservoir, bubbles of azote rise from time to time in great abundance. The springs of Mariano have a temperature of 44° c., but in particular spots it is from 56° to 64°. They contain a very little sulphuretted hydrogen. They also rise from gneiss, and evolve azote. Silica is the predominating ingredient in solution. Their height above the sea is 476 metres.—*Ann. de Chim. et de Phys.* xxiii. 272.

### PHYSIOLOGY.—*On some recent Discoveries relative to the Nervous System. By* M. Magendie.

M. Magendie offers some proofs and illustrations of Mr. Charles Bell's beautiful investigations, on the distinction between the nerves subservient to sensation and motion. An individual had lost the use of his two arms for several years, but he had retained a lively sensibility in these parts. He died, and on examining his body, the posterior roots of the brachial nerves (as they issue from the spine) were perfectly sound, while the anterior roots were evidently altered, had lost their medullary substance, and were reduced to their membranous sheath. The nerves give sensibility or mobility to our organs, only because they are connected with the spinal marrow; whenever they are insulated by a wound, or any other cause, the part to which they go becomes motionless and insensible. It was, therefore, of consequence to know if the spinal marrow was not itself divided into two halves, the one destended to motion, the other to feeling. M. Magendie has discovered that the spinal marrow is formed, as it were, of two cords juxta-posited, one of which is endowed with an exquisite sensibility, while the other is, so to speak, a stranger to the property, and appears to be reserved for motion. Since it is shewn by the fine experiments of Legallois, that all the other organs, without exception, derive from the spinal marrow their sensibility and mobility, we are led to the remarkable conclusion, that we must cease to seek for any one point in the whole body where the sensibility and mobility are compounded together. Hence it seemed very probable that, in persons who lose the power of moving, while they retain their sensibility, and that reciprocally in those who lose sensibility retaining mobility, there is a disease in the one case of the motive cord of the spinal marrow, and in the other of the sensitive. A lunatic of the hospital of Charenton, had lost, for seven years, the faculty of motion in the whole body, although he retained its sensibility. He died last month. M. Royer Collard, physician to the establishment, made the spinal marrow be examined with the greatest care, and found, in fact, a very marked alteration in the whole motive portion of the spinal marrow, while the portion where sensibility resides was perfectly sound. The centre of the spinal marrow is devoid of sensibility; on touching it, no movements are excited in the body. It is on the surface of this organ, that its properties are developed under the double relation of movement and feeling. Those who think that the electric fluid circulates habitually in our nervous system, may derive from this fact a new argument in favour of their opinion; for electricity diffuses itself, as is known, on the surface of the bodies which it pervades. It is unnecessary to remark, that the facts above related, should have a great influence in the treatment of different palsies. When the cerebral hemispheres of any animal are put out of condition for acting, the animal runs straight forward, with singular rapidity, as if it were pursued. We might say, that an irresistible force presses and precipitates it. If, on the other hand, the action of the cerebellum be stopped, the movements take an entirely opposite direction. The animal draws back; and it is a remarkable phenomenon to see a bird, for example, whose cerebellum has been slightly touched, for whole days make no attempt to walk, swim, or fly, unless it be backwards. It would seem, therefore, to result from these experiments, that an animal in the ordinary state of health, is placed between two forces, which make an equilibrium, of which one would push it in advance, while the other would push it backwards. Volition would have the power of disposing at its option of these two forces.

A disease of the horse, little known, was proper to verify the precision of these results. Veterinary surgeons call this disease *immobility;* and, in fact, when it is wished to make the animal seized with it, fall back, whatever effort be employed, and whatever means be taken, it stands motionless. The forward movements are, on the contrary, easy, and seem sometimes to occur even without the participation of the will. If the inference which I have drawn be exact, the disease ought to consist in a physical alteration of the cerebrum, or in some obstruction of the action of this organ. I caused to be examined, last month, two horses attacked with immobility, and the conjecture has been completely verified. In both the cerebrum was visibly altered; the cerebellum, on the contrary, was unaffected. It appears, then, to be demonstrated, that the two opposite motive forces of the cerebrum and cerebellum exist in animals, and that, in certain cases, they may be withdrawn from the influence of the will. M. Magendie relates a case of a man somewhat similarly affected, who was cured by some grains of sulphate of quinina.—*Ann. de Chim. et de Phys.* xxiii. 429.

### *Prussiate of Iron as a Cure of Intermittents.*

Doctor Zollickoffer, of Baltimore, has employed this substance, and his success has been as remarkable as with cinchona.—*Journ. de Pharm. July,* 1823.

### *Injection of a Solution of Opium into the Veins of an Hysterical Patient. By* Charles W. Coindet.

This experiment was made at Edinburgh, and the result was such as to deter any young physiologist from repeating it in hysteria. The patient was seized with violent spasms, constituting a case of idiopathic tetanus. They commenced very regularly by attacks of emprosthotonos, the head frequently striking the knees with force. Opisthotonos succeeded; the body took the form of a bow, and rested only on the heels and occiput. All the muscles of the body participated in this state of painful tension, which, one time, lasted twenty-seven minutes. The respiration was performed with difficulty, the pulsations of the heart became feeble and irregular, and the young girl (fourteen years of age), was threatened with suffocation. This horrible agony was succeeded by some convulsions of pleurosthotonos, which terminated the paroxysm. Dr. Coindet dissolved a scruple of common opium in an ounce of distilled water, heated to the temperature of 80° cent. At half-past seven in the evening he began the injection, assisted by his friends MM. Hercy and Lucius O'Brien. He made an opening in the right basilical vein, with an ordinary lancet as for blood letting. He removed the bandage from the arm; he then introduced the pipe of a syringe, and threw a drachm and a half of the solution into the vein, taking care to exclude every portion of air, though the experiments of Nysten had shewn that a few air bubbles would occasion no mischief. The breathing was immediately affected, becoming more regular, less rapid, and less convulsive. The pulse and other symptoms remained as before. The successive injections were repeated at intervals of five minutes. At the second, the breathing became quite natural; the pulse rose to 100, and was fuller. The skin became slightly coloured, and was soon covered with a faint perspiration. The spasms lost their violence; she heaved one or two sighs, like a person coming out of a profound sleep. After the fourth injection she recovered her hearing, but not her sense of sight. At the fifth, she began to see, and articulated some phrases distinctly. The operation was not followed with any disagreeble symptoms. On the following day, the girl described her sensations with much clearness. At every injection, it appeared as if a torrent of fire had been poured into her veins, which rising up her arm, and following the course of the vessels, which she pointed out very exactly, passed under the clavical of the same side, and concentrated its operation for some instants on the chest, whence it proceeded to the head and along the back, from which it diffused itself through the whole system, and produced lively prickings and an intense heat in the skin. She spoke of her sensations as having been very painful. After six weeks of convalescence, she relapsed into a similar state of disease to that for which the injections were used. She finally recovered from the convulsive affections by sea bathing; but was afterwards seized with swelling of the mesenteric glands.

Dr. Coindet says, we must not expect from opium injections any thing more than the temporary cessation of the spasms, whereby the stomach may be brought back to its natural functions, which interval must be taken advantage of, for administering the suitable remedies, by the customary passages.—*Bibliotheque Universelle, May* 1823.

ECONOMICS.—M. Viney one of the editors of the *Journal de Pharmacie*, has given, in the number for February last, the following recipe for making a fetid and bitter solution, capable of destroying all kinds of insects:—

| | |
|---|---|
| Take of wood mushrooms, or large brown fetid boletuses | 6 pounds |
| Black soap . . . . | 2 ——— |
| Grated nux vomica . | 2 ounces |
| Water . . . . . | 200 pounds |

The mushrooms bruised and beginning to putrefy, are to be put into the water holding the soap in solution. The mixture is to be left to putrefy in a cask for some days, care being taken to agitate the liquid from time to time. When it has become very fetid, the decoction of the nux vomica in water is to be poured in. This liquor is employed to sprinkle the objects from which insects are to be repelled, whether in gardens or elsewhere, taking care not to use it on gildings or polished metals, which it would blacken. The insect cannot stand this fetid poison.

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## Art. XIV.—MISCELLANEOUS INTELLIGENCE.

### I. Mechanical Science.

1. *Remarks on Iron Wire Suspension Bridges.*—The following remarks on this subject are from a memoire by M. Dufour, the Engineer of the Geneva bridge, briefly mentioned at page 369 of the last volume of this Journal: they are naturally connected with the account of that bridge.

Speaking of the comparative strength of iron in wires and in bars, (see p. 367 last vol.,) M. Dufour says, " The immense advantage of employing iron in wire rather than in bars, is thus rendered evident: it is more manageable, its strength is double, the strength may be better proportioned by putting the number of wires necessary to the resistance required, and a certainty is obtained of the state of the interior parts of the suspending lines, which nothing can give when large bars are used."

" It appears at first that the minimum of the force of the wire should be calculated upon, and not the mean; but as each bundle contains many wires, although there may be some of a smaller strength, there will be others that will surpass in strength, and thus the mean should be used in estimating the strength of the whole, although in employing a single wire the minimum only ought to be taken."

With regard to the Geneva bridge, M. Dufour says that after a period of four months in which the bridge had been in full use, it had not suffered the slightest alteration in its primitive form. The path has retained the degree of curvature given it at first, and no sensible lengthening of the wires has occurred. The bridge, however, has been well tried, curiosity has taken great numbers of persons on to it at once, and all the large stones required in the latter part of the work, were taken over it on carriages without the slightest damage. The elasticity of the bridge is also what it was at first, a man walking with a moderate step does not at all disturb the steadiness of the path; on walking quickly there are slight vibrations produced, but no oscillations, and the vibrations are such as never to be communicated from the one bridge to the other, or in any way to affect the masonry.

The expense of the bridges was as follows :—

| | |
|---|---|
| Masonry of the abutments, *&c.* . . . . | 4100 francs. |
| ———————— lodges, stations, *&c.* . . | 3800 |
| Forged iron, *&c.*, for the gates . . . . | 2800 |
| Iron wire and workmen . . . . . . . | 1940 |
| Wood-work required, workmen, *&c.* . | 2250 |
| Lead, copper, tin, varnish, *&c.* . . . | 800 |
| Terraces for the parapets, foundation, *&c.* | 160 |
| Various expenses . . . . . . . . . | 500 |
| | 16,350 |

*Bib. Univ.* xxiv. 297.

2. *Test for the action of Frost on Building Materials, by* M. P. Brard.—MM. Lepeyre and Vicat knowing that I had been long occupied in the study of mineralogy as applicable to the arts, engaged me in an investigation of the means best adapted to distinguish such stones, as, being otherwise fit for building materials, gave way to the action of frost. I found it impossible in this respect to ascertain any thing from their mineralogical characters, and was obliged to follow another course. During the winter of 1819, I carefully examined with a lens the chalky limestone of the neighbourhood of Perigueux, and the sandstone of the coal -basin of la Vezère, both equally liable to this action, I soon found that each scale of the limestone, and each grain of the sandstone was raised by the re-union of small needles of ice, which when they melted suffered the particles to fall and collect about the stone, and that where particles had fallen off in this way, a fresh succession was raised in the same manner, and ultimately separated from the mass.

I was struck by the resemblance of the ice in silky crystals to the saline efflorescences which appear between the plates of certain shists and on the surface of old walls. I remembered the effect of common salt on bad pottery, and on the saline rocks of the Tyrol, and conceived the idea of substituting the action of a saline solution to that of common water. After various experiments, I gave the preference to sulphate of soda, its effects being the most constant and most comformable to the action of frost.

The experiment that it may lead to satisfactory results should be conducted as follows. Suppose an excavation newly made into limestone or other rocks, and it be desired to ascertain the liability of the rock to disintegration by the action of frost.

1st. A cube of two inches in the side is to be cut from each part to be tried; the various cubes numbered with thick China ink, and their original sites also marked.

2nd. About four pints of common cold water is to be saturated with sulphate of soda, so that a few grains of the salt shall remain undissolved.

3rd. This solution is to heated to ebullition, and then all the cubes to be entirely immersed in it. When the boiling has recommenced it is to be continued for half an hour.

4th. The cubes are to be withdrawn from the solution and placed each one in a saucer, numbered as the cube is; a small quantity of the solution is to be poured on to each cube, and the whole left until covered with white efflorescences perfectly analogous in appearance to the rime or hoar frost, which causes the disintegration of the stones. These efflorescences will appear in about twenty-four hours if the air is dry or hot, but in a humid atmosphere are sometimes five or six days.

5th. When the efflorescences appear on the angles and sides of the cubes, they are to be dissolved again by means of a few drops of water, or better still with a little of the solution in which the cubes were boiled. If well managed the efflorescences will soon re-appear, and when well formed, are again to be removed in a similar way, and this is to be repeated for three or four days together*; after which each cube may be washed with abundance of common water, but without removing it from the saucer.

6th. The specimens to be tried having been washed on all their faces, the detached matter is to be examined, and a judgment formed from it, of the relative qualities of each kind of stone submitted to the proof: for the greater the number of the detached particles collected in the saucer, the more liable is the stone to be attacked by frost; the smaller the number the more capable is it of resisting the action.

As yet, all the results of this test have accorded perfectly with the effect of time and frost. Such stones as have been found to disintegrate by frost have given way to the salt, such as time has sanctioned have resisted the new agent, so that the mechanical effects of the two are perfectly analogous. Crystallization takes place with both, augmentation of volume, efforts on the surfaces of the small cavities containing the water or solution, and if the aggregation be not sufficiently powerful to resist the action, disruption, and a gradual decay of the rocks either in their natural sites, or if they have been applied to use in their new situations. The action of the sulphate of soda being quite mechanical, is exerted indifferently on all kinds of rocks deficient in aggregation, on limestones, sandstones, large grained granite, granites of too micaceous a structure, shists, lavas, *&c.* It may be employed as a proof or test also even upon slates, bricks, tufas, mortars, and cements, as is proved by a table of various results of this kind.

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* If the proof be continued for a longer period, good building stones may be rejected, for the prolonged action of the salt is more powerful than that of ice.

The above is freely translated from a paper inserted by M. Brard, in the *Bib. Univ.* xxiv. 224.

3. *On the Strength of Cast Iron and other Metals.*—It was our intention to have noticed a new edition of Mr. Tredgold's valuable essay on the above subject, in a former part of our Journal, but this has been prevented by the pressure of other matter. We must, therefore, rest satisfied with laying before our readers the contents of the eleven sections into which the work is divided, reserving to a future occasion a more explicit account of its contents.

The *First Section* consists of introductory remarks on the use and the qualites of cast iron; and of cautions to be observed in employing it. This section is followed by three extensive tables, which will often save the practical man a considerable share of trouble in calculation.

The *Second Section* explains the arrangement and use of the tables, which precede it; and in this edition, the number of popular examples is much increased.

It is a common and a well understood fact, that an uniform beam is not equally strained in every part, and therefore may be reduced in size, so as to lessen both the strain and the expense of material.

The *Third Section* points out the value of cast iron, in this particular, and the forms to be adopted for different cases.

The *Fourth Section* contains a popular explanation of the strongest forms for the sections of beams; the construction of open beams; and the best forms for shafts. A due consideration of these two sections will enable the young mechanic to guard against some common errors in attempting to apply these things to practice. They are much augmented, and a new principle of constructing bridges is explained in the fourth section.

The *Fifth Section* is wholly devoted to experiments on cast iron; it will be found to contain, in addition to the author's experiments, almost all of the experiments that have been described by preceding writers.

To this section a great many new experiments have also been added, to show the relative strength of iron of different qualities; and also seven new experiments on torsion, made by Messrs. Bramah. The section concludes with the result of the author's observations on the relation between the appearance of the fracture and the strength of cast iron as determined by experiment.

The *Sixth Section* contains experiments on malleable iron and other metals, and is entirely new. The effect of hammering and the decrease of force by heat, are experimentally examined; and the cause of English iron being inferior to Swedish, for particular purposes, is pointed out.

In the *Seventh Section* we are shown how to obtain some of the most useful practical rules from the first principles that are furnished by experience.

The *Eighth Section* treats of the stiffness to resist lateral strains, with its application to some interesting practical cases.

The *Ninth Section* is on the strength and stiffness to resist torsion or twisting, with its application to machinery.

The *Tenth Section* treats of the strength of columns, pillars, and ties, with some new examples. It may be useful to remark, that the most refined methods of analysis have been applied to the same subjects by Euler, Lagrange, and other continental mathematicians, without arriving at results more accurate, more simple, or more convenient in practice.

In the *Eleventh Section* the author considers the resistance of beams to impulsive force. In this section will be found many important rules, with examples of their application to the moving parts of engines, bridges, *&c.*, wherein the advantage gained by employing beams of the figures of equal resistance is shown.

The Eleventh Section is followed by an extensive *Table of the Properties of Materials, and other Data, often used in Calculations*, arranged alphabetically, and in this Edition much enlarged. By means of this table the various rules for the strength of cast iron, contained in this work, may be applied to several other kinds of materials.

A Note, added at the end of the table, on the chemical action of some bodies on cast iron, will be read with interest by those who employ cast iron where it is exposed to the action of sea water.

4. *On the Capillary Action of Fissures, &c.*—M. Dobereiner has remarked a singular effect produced apparently by fissures. Having filled a large glass flask with hydrogen, and left it standing over water, it was observed some days after, that the water had risen in it above one-third of its capacity. The only cause for this effect that could be assigned was, the existence of a very minute fissure in the glass. Filled a second time and left over water, the fluid had risen in it above an inch and a half in twelve hours, and in twenty-four had risen two inches and three quarters, during which time the barometer and thermometer had not sensibly altered. In other experiments, vessels of other forms were used, and the water uniformly rose in those having fissures.

When one of these vessels filled with hydrogen was covered by a bell glass, or when the vessels were filled with atmospheric air, oxygen, or azote, instead of hydrogen, no change took place.

M. Dobereiner considers the effect as due probably to capillary action. He suggests that all gases may be considered as consisting of solid atoms of various sizes, enveloped by atmospheres of heat also very different, and that hydrogen though it has the largest atmosphere of heat, has the smallest atom, and is thus permitted to escape by fissures, which retain the other gases. "Probably," he says, "fissures may be formed which will permit azote to pass, but not oxygen, and others again which will let the oxygen out, but not carbonic acid gas."

Another experiment which seems related to this subject is as follows:—A thermometer-tube had been drawn out very fine in the lamp, and it being desired to have it filled with alcohol, the point was immersed in that fluid, and the bulb heated until no more bubbles of air escaped; the tube was then cooled, but no alcohol entered. When again heated abundance of bubbles of air passed out through the alcohol, though when recooled no alcohol would enter. Upon examining the tube with a lens, nothing was seen which could prevent the entrance of the alcohol; on withdrawing the tube from the alcohol, the external air entered with a hissing noise. M. Dobereiner conceives that the diameter of the tube was so small that the alcohol could not enter, but only the air which it contained.—*Ann. de Chim.* xxiv. 332.

5. *Sound produced by opening a Subterraneous Gallery.*—In the road made by Napoleon communicating between Savoy and France, and which passes by Chamberry and les Echelles, there is, as is well known, about two miles from the latter place, a gallery cut in the solid rock, twenty-seven feet high and broad, and nine hundred and sixty feet in length. Mr. Bakewell states in his travels, that this gallery having been commenced at both ends, when the excavations from each end nearly met, and the thin partition of rock between them was first broken through by the stroke of the pick, a deep and loud explosion followed resembling thunder. The cause of this explosion Mr. Bakewell thinks is easily explained. The air on the eastern side of the mountain being sheltered both on the south and west from the sun's rays, must be frequently many degrees colder than that on the western side. The mountain rises full one thousand feet above the passage, and at least fifteen hundred feet above the bottom of the valley, forming a partition between the hot air of the valley, and the cool air of the ravines on the eastern side, and a sudden opening being made for the dense air to rush into a rarer medium, must necessarily produce a loud report, just as a bladder does upon bursting in the rare air of a receiver. The sound of the explosion being greatly increased by reverberation through the long archway on each side.—*Bakewell's Travels.*

This explanation of the origin of the sound seems insufficient to us, inasmuch as it would require a much greater difference of barometric pressure on the opposite sides of the previously existing partition of rock than probably existed.

6. *Nautical Eye-tube.*—A trial has been made on board the Clio among the Orkneys, and in the Moray Frith by Mr. Adams, of the performance of his eye-tube to the telescope of a sextant for taking altitudes when the horizon is invisible. In making the observations the horizon was always screened from the instrument, and under these circumstances after rejecting a few observations the mean difference of 199 altitudes of the sun, moon, and stars, taken by the eye-tube, from those taken at the same time in the ordinary way by the officers of the Clio, and corrected for dip, amounted to only 1′ 10″. The altitudes taken by the eye-tube are not affected by any dip or depression of the horizon. Considerable care and practice is required in the use of the instrument, but that attained, the latitude, the time at the ship, and consequently the longitude may all be determined by it when the horizon is invisible. By means of it also either the large or the pocket sextant may be employed on shore as a substitute for the theodolite, upon making the necessary allowance for the parallax of the instrument in the name of index, error, which on becoming sensible, must vary inversely with the distances of the reflected terrestrial objects.—*N. M. Mag.* xii. 16.

7. *Leghorn Straw Plait.*—The Dublin Society having offered premiums for the best imitations of Leghorn plait, awarded three prizes to successful candidates. Not less than twenty-four specimens were exhibited from widely remote parts of Ireland. The finest specimen was made from *avena flavescens*, or yellow grass, by Miss Collins of Plattin, near Drogheda. The second was made of *cynosurus crystatus*, or crested dog's tail, by Miss Grimley of Kiltinon, near Newton Mount Kennedy. The third of *agrestis vulgaris*, or common bent grass, by Miss Campbell of Londonderry.

## II. CHEMICAL SCIENCE.

1. *On Fulminating Silver and Mercury.*—The following results are collected from a memoire on these substances, by Dr. Just Liebeg, which has appeared in the *Annales de Chim.* xxiv. 294.

The fulminating silver was obtained by dissolving about 60 grains of fine silver in half an ounce of nitric acid, spec. grav. 1.52, adding two ounces of alcohol of spec. grav. .85, and heating slowly in a flask until ebullition commenced; in a short time, white crystalline flocculi appeared, the vessel was removed from the source of heat, and left to cool. The ebullition continued some time, and the deposit augmented. The vessel should not be cooled hastily in this process, as great loss of the compound is occasioned.

Thus prepared, the fulminating silver appears in white silky acicular crystals, possessing the well known detonating properties, perfectly soluble in 36 parts of boiling water, and re-crystallizing as the solution cools. It has a metallic taste, stains the skin, if exposed to air becomes first red, then black, and to test papers appears as a neutral salt.

Fulminating mercury was prepared according to Howard's process: 100 grains of mercury being dissolved in half an ounce of concentrated nitric acid, and two ounces of alcohol added. Heat is then to be applied, as in the former case. At first a little nitrate of mercury is deposited, but is soon redissolved, and then on a sudden the liquor becomes grey from the reduction of part of the oxide of mercury, and the liberation of a dense vapour, occasioned by the volatilization of a portion of mercury with the ether that rises. After some time the liquid becomes yellow, and dendritical crystals appear, which augment on cooling until nearly a quarter of an inch in length. They are greyish-white, harsh to the touch, and heavy, but when purified by being dissolved and crystallized two or three times, appeared as perfectly white brilliant silky crystals, having a mild metallic taste, and detonating violently by a blow. They are pure fulminating mercury.

On adding lime-water to fulminating silver the latter dissolved, leaving a little black oxide of silver; when a few drops of nitric acid were added to the clear solution a white precipitate fell, which detonated like the original compound. It now dissolved without any residue in lime-water, and was precipitated again by acid, as before, without any indication of decomposition.

Substituting potash for lime-water, and boiling, exactly the same effects took place. The fulminating silver combined also in the same manner with magnesia, baryta, strontia, soda, and ammonia, and with all of them presented the same phenomena, except that ammonia did not cause the separation of oxide of silver. The quantity of oxide separated by the alkaline bases from 100 of fulminating silver was 31.25.

Thus it appears that fulminating silver perfectly resembles a compound salt; its acid combines with the alkalies, *&c.*, and its base, the oxide of silver, separates; and in confirmation of this view of its nature it was found that compounds of the acid, and all other bases, might be obtained perfectly definite and crystallized, and possessing strong detonating properties.

A quantity of fulminating silver was decomposed by lime, the liquid filtered, concentrated, and carefully precipitated by nitric acid, excess of the latter being avoided. The new acid, when well washed, appeared as a white powder, very soluble in boiling water, reddening litmus paper, and crystallizing on cooling.

Researches were then made into the nature of this acid: the term *fulminate*, has been applied to the salts containing it. Muriate of potash added to fulminate of potash produced no precipitate of chloride of silver; but muriatic acid decomposed the salt, and chloride of silver, muriate of ammonia, hydrocyanic acid and carbonic acid, resulted. Fulminate of potash does not precipitate persulphate of iron, nor does the addition of muriatic acid form a prussiate of iron. Metallic copper precipitates all the silver from fulminate of potash, and a plate of zinc indicates the copper present; but excess of potash does not separate the copper, nor does the fluid become blue by adding ammonia, though when the solution is decomposed by muriatic acid, the copper is easily found by those tests. Chromates, prussiates, and carbonates, do not precipitate the silver from alkaline fulminates; these properties point out a strong analogy between this acid and the metalliferous cyanic acids.

The fulminating acid boiled with oxide of silver, gave fulminating silver; boiled with oxide of mercury, it produced a compound in small brilliant plates.

Conceiving from analogy that the acid of fulminating mercury differed from that of fulminating silver in the substitution of the former metal for the latter, experiments were made to ascertain this point; crystals of fulminating mercury boiled with potash, deposited oxide of mercury, and the fluid, when precipitated by nitric acid, gave a white precipitate, which, when dry, detonated strongly by percussion; with baryta, strontia, and lime, similar compounds to those formed by fulminating silver, were produced. The separation of the acid from fulminating mercury does not always succeed. In only two operations out of eight was the acid obtained in yellow detonating crystals.

A quantity of fulminating silver was put with metallic mercury into water and boiled; after some time the liquid became turbid, it was filtered, and furnished crystals exactly the same as those produced by the acid of fulminating silver and the oxide of mercury. Boiling another portion for a much longer time, the precipitate deepened in colour, and when no more was formed, the whole was filtered and crystallized. The crystals were very fine, and pure fulminating mercury; and an amalgam of mercury and silver remained. The reverse operation was performed of preparing fulminating silver from fulminating mercury; the latter was boiled with silver which had been precipitated from the nitrate by copper, and to which a quantity of platina filings had been added; by the galvanic action of the two metals the mercury was precipitated, and the silver dissolved. The experiment requires rapid manipulation and simple decantation, otherwise the crystals will always contain mercury.

Fulminating silver was boiled with copper; the silver precipitated, and the liquid, which was found to contain copper, after some time deposited a bluish-green powder, which behaved like a true combination of oxide of copper with the acid of the fulminating silver, containing copper in place of silver. The compound detonated more feebly than that of silver, and was difficultly soluble in boiling water; on evaporating the mother liquor, a large quantity of fulminating copper was obtained. Zinc gave similar results, but more rapidly. Iron also produced a crystallized fulminating compound.

When fulminating mercury was acted on by the metals, similar phenomena were produced; and fulminating copper and fulminating mercury were thus obtained.

When fulminating silver was boiled with magnesia, the liquid was found to contain but very little of the acid, but a reddish precipitate had formed, which, though it contained the greater part of the fulminating mercury, merely decrepitated feebly when thrown upon a hot coal. Half an ounce of it heated in a retort, decomposed quietly, yielding a portion of carbonate of ammonia and water, and carbonic acid gas, no other gas being liberated. In order, therefore, to obtain a knowledge of the constituents of fulminating silver, 100 parts were well mixed with 400 parts of calcined magnesia, and heated in a luted retort, the products being carefully received and estimated. They were,

| | With fulminating silver | With fulminating mercury |
|---|---|---|
| Carbonic acid | 35·5 | 25·8 |
| Ammonia | 13·7 | 10·0 |
| Water | 7·2 | 5·2 |
| Silver | 41·0 | Mercury 56·9 |
| Loss | 2·6 | 2·1 |
| | 100· | 100· |

These being the mean of four experiments on each compound. The only substance which varied was the carbonic acid, and the proportions of the other substances remained constant. These gave as the ultimate elements,

| | Fulminating silver | Fulminating mercury |
|---|---|---|
| Oxygen | 32·22 | 23·39 |
| Hydrogen | 3·22 | 2·34 |
| Nitrogen | 11·28 | 8·23 |
| Carbon | 9·68 | 7·04 |
| Silver | 41·00 | Mercury 56·90 |

The following are some of the compounds of the acid of fulminating silver with bases.—*Magnesia* combines in two proportions with the acid: one is a rose-coloured powder, not soluble or detonating, but merely decrepitating by heat; the other is in beautiful white filamentous crystals, resembling capillary silver, and strongly detonating.—*Baryta* combines with the acid apparently in two proportions; the first crystallizes in dull white grains, which detonate powerfully, and are difficultly soluble in water.—*Strontia* resembles baryta in its compounds.—*Zinc* forms a salt in small yellow granular crystals, very soluble, and very heavy.—*Potash* produces a salt which crystallizes in long white brilliant plates, having a disagreeable metallic taste, not affecting test-paper, dissolving completely in eight parts of boiling water, and detonating powerfully when heated or struck. It contains 85·08 of acid, and 14·92 of base.—*Soda* has always produced a salt in small rounded plates, brown and brilliant; they are lighter and more soluble than the preceding, but otherwise resemble it. They contain 88·66 of acid, and 11·34 of base.—*Ammonia* with fulminating silver leaves no residuum. Berthollet's compound being formed at the same time with the other. On cooling, a large quantity of granular crystals are obtained, which are difficultly soluble, and have a strong metallic taste. They detonate even in the liquid when touched by a glass rod, but fortunately if excess of alkali be present the detonation does not extend to the neighbouring portions.

2. *On the unequal Dilatation of a Crystal in different directions, by heat.*—On measuring the mutual inclinations of the planes of a crystal of carbonate of lime at different temperatures, M. Mitscherlich observed that they varied sensibly with the temperature, the variation sometimes amounting to 8′.5 from 32° to 212. Fahr. When the temperature rose, the obtuse diedral angles diminished, or in other words the short axis of the rhomboid expanded more than the other diagonals, so that its form approached to that of the cube. M. Mitscherlich concluded, therefore, that the double refraction of the crystal would at the same time diminish; a result confirmed by an experiment which he afterwards made with M. Fresnel in the manner adopted by that philosopher in 1817, to render more sensible the changes in the tints of plates of sulphate of lime. M. Fresnel had then observed, that elevation of temperature sensibly diminished the double refraction of sulphate of lime; and according to the recent experiments of the two philosophers the same effect is produced, though in a much less degree, on rock crystal. This experiment, however, requires repetition.

It appears, therefore, that generally an uniform elevation of temperature in a crystal diminishes its double refraction. M. Mitscherlich thinks that heat ought always to separate the molucules of a crystal farthest apart in that direction in which they are most contiguous.—*Ann. de Chim.* xxv. 109.

3. *Difference of crystalline Forms of the same Substance.*—M. Mitscherlich, who first observed the remarkable fact that a body may affect two different crystalline forms, has, in a memoir on this

subject, quoted sulphur as an instance. Natural crystals of sulphur are furnished by some calcareous strata, and by volcanoes. Artificial crystals may be obtained either by evaporating a solution of it in carburet of sulphur, or by fusion of the sulphur and slow cooling. On fusing native sulphur, it gives the same crystals as common sulphur. The primitive form of the crystals of sulphur, either natural, or obtained as above by evaporation, is an octoëdron, with a rhombic base; but the primitive form of the crystals obtained by fusion, is an oblique prism, with a rhombic base.—*Ann. de Chimie*, xxiv. 264.

4. *Supposed Effect of Magnetism on Crystallization.*—The following is an experiment first made by Professor Maschmann, of Christiana, and confirmed by Professor Hanstein, of the same city; we should not have noticed it but for these names. A glass tube is to be bent into a syphon, and placed with the curve downwards, and in the bend is to be placed a small portion of mercury, not sufficient to close the connexion between the two legs; a solution of nitrate of silver is then to be introduced until it rises in both limbs of the tube. The precipitation of the mercury in the form of an arbor Diana will then take place, slowly only when the syphon is placed in a plane perpendicular to the magnetic meridian; but if it be placed in a plane coinciding with the magnetic meridian, the action is rapid, and the crystallization particularly beautiful, taking place principally in that branch of the syphon towards the north. If the syphon be placed in a plane perpendicular to the magnetic meridian, and a strong magnet be brought near it, the precipitation will recommence in a short time, and be most copious in the branch of the syphon nearest to the south pole of the magnet.

4. *On Thermo-magnetism.*—The following account of results on the magnetism of a single piece of metal developed by heat, is abstracted from a paper by Dr. J. d'Yelin, or rather from an account of that paper in the *Bibliotheque Universelle.* The results, if confirmed by further experience, are very highly important to the theory of magnetism.

In repeating the experiment of Seebeck, M. Yelin made use of platina, gold, silver, iron, copper, brass, zinc, tin, lead, antimony, bismuth, and arsenic. The result of his observations was that "the effect of Seebeck's circuit should not be considered as a determinate function of power possessed by the heterogeneous metals of developing electricity by contact, and of their various conducting powers as to heat; and that therefore, conclusions cannot be drawn from these properties," as is proved by the following facts;

1. Silver and zinc give by contact an electricity stronger than silver and antimony; but a circuit formed of the two latter metals

has much greater power than one composed of the former, which is very feeble. The case is the same with the two sets, copper and zinc, and copper and bismuth.

2. Brass, copper, and lead, according to Bockmann, have a conducting power as to heat of 344, 346, and 850; nevertheless, a circuit of brass and copper is sensibly stronger in its action than a circuit of lead and copper.

3. Finally, silver in contact with antimony is electrized negatively, in contact with zinc it is still more powerfully so; but other circumstances being equal, a circuit of antimony and silver has seven times the power over a magnetic needle that a circle of zinc and silver has. Antimony is positive when opposed to platina, gold, or silver, and negative when opposed to copper, tin, lead, or zinc; but whichever of those metals be formed into a circuit with antimony, the same effect is obtained, the same pole of the needle always being urged to the same side. Bismuth and antimony are both positive when in contact with platina, gold, and silver, but all other things being equal, as the dimensions of the metal, the soldering, the temperature and arrangement, a circuit formed of bismuth and one of the last named metals, turns the pole of the needle 14°, 51°, or 45° to the east, whilst if antimony be substituted for the bismuth, the pole is thrown 18°, 25°, or 30° to the west.

Being induced to consider the rupture of the equilibrium of temperature as the principal cause of the electro-magnetic action of Seebeck's circle, M. d'Yelin endeavoured to obtain similar effects with a single piece of metal, and having obtained very decided effects, he has given to this class of phenomena the name of thermo-magnetism. That very feeble magnetic action might be observed, very delicate needles were used; they were of great tenuity and suspended by a single spider's thread.

If a band of any single metal be formed into a circuit of any figure, by riveting one of its ends near the other, and the projecting end be heated by a flame, whilst the circuit is plunged in cold water, this band will become electro-magnetic, and its properties may be easily ascertained. The experiment was made with zinc, bismuth, brass, tin, lead, and copper, and M. d' Yelin infers that "all metallic bodies acquire electro-magnetic properties when their various parts are unequally heated, and that the action is stronger as the difference of temperature is greater."

This fundamental experiment being established, the following are the principal results obtained by the author:—

I. The metals, in reference to their thermo-magnetic properties, may be ranged as follows, commencing with those which possess them in the highest degree, bismuth, antimony, zinc, silver, platina, copper, brass, gold, tin, lead.

II. A metal acts differently on the needle according as the hot or the cold part of it be placed under the needle. The following experiments were made with cast bars six or seven inches long, one inch in thickness, and formed either as cylinders, or as prisms with three, four, or six sides; solid and hollow balls were also employed:—1. If one extremity of a bar of bismuth be heated, the bar be placed in the direction of the magnetic needle, with its cold end to the north, and the hot end be brought under the needle, the point of the needle will turn towards the east. 2. If the direction of the bar being preserved, it be moved towards the south until its cold end is under the needle, the needle will turn towards the west. 3. The inverse effects are obtained when the hot end of the bar is towards the north. 4. When the bar is heated in the middle, and the ends preserved cold, the same effects are obtained for each half of the bar. 5. The magnetic effects are sensible when one part of the bar is heated merely by the hand and the other cooled by snow.

III. The magnetic action of metals unequally heated depends on the form given them in casting, and in this it differs from the action of Œrsted's connecting wire. 1. If an equilateral triangular prism of bismuth be used as in the former experiments (1), and its faces be turned upwards successively, one of its faces will make the needle deviate to the east, the next face (that towards the east) brought into the place of the first, will make the needle deviate to the west; the third face has so uncertain an effect that it may be considered as null. 2. If a square or four-sided prism of bismuth, antimony, or zinc, be used in a similar manner, it will be found that two contiguous faces when turned upwards will make the needle move eastward, whilst the other two faces will move it westward, so that the prism may be considered as composed of two triangular prisms of which the un-magnetic faces are in contact. 3. With a regular hexagonal prism three of the faces move the needle eastward, and three move it westward. 4. Cylinders present peculiar effects; a cylinder of bismuth had been thrown with its mould into cold water immediately after being cast, another was suffered to cool slowly; when these cylinders were used in place of the prisms, the ends which were uppermost in the moulds being placed under the needle, one part of the curved surface urged the needle to the east, and the other part to the west; these parts were equal in the first cylinder, but unequal in the second. When the other extremities of the cylinders were placed under the needle, then the curved surface of the first cylinder presented four nearly equal portions which successively turned the needle to the east and west: the second bar presented six similar portions.

The differences remarked between the extremities of the cylinder, and also between the cylinders themselves, when cooled slowly or rapidly, induces M. d' Yelin to conclude there is some relation between the crystallization of metals and their magnetic properties.—*Bibliotheque Universelle*, xxiv. 253.

5. *Electromagnetic Multipliers.*—Dr. Kaerntz has lately been occupied in proving experimentally the amount of the advantage obtained in electromagnetic multipliers, by each additional circumvolution of the wire. His motor was a zinc plate about eight inches long and four inches wide, the copper opposing both sides was consequently double that size. The fluid conductor was a solution of muriate of ammonia in spring water, with the addition of one hundredth of sulphuric acid. The connecting wire was copper harpsichord wire, covered with silk thread, and the same length was used in every experiment. By connecting the plates with the wire before immersion, by immersing slowly and by other expedients, any important variation in the intensity or quantity of action was avoided.

In this way it was found that the quantity of power of the instrument over the needle, was exactly in proportion to the number of convolutions, six convolutions giving six times the power of one convolution; and by experiments, when the forces of the instrument and of the earth's magnetism were arranged in different ways, this result was confirmed. Such an instrument is therefore more correctly called a multiplier than a condenser.—*Phil. Mag.* lxii. 441.

6. *Plate Electrical Machines.*—A variation in the construction of plate electrical machines has been devised and practised by M. Metzger of Siblingen in Schaffhouse, which would seem to be a real improvement. Considering that the effect desired in using the machine was first highly to excite the glass, and then to collect the electricity from it, M. Metzger concluded that the distance between the rubber and the points of the conductor in machines of the common construction was injurious in its effect, not only by causing the dispersion in part of the electricity excited, but by uselessly wasting the exciting surface. Plates were therefore mounted in a very compact and perfect manner, with three pairs of rubbers placed at equal distances from each other; the conductor also had three arms furnished with points a little in advance of each pair of rubbers, to collect the electricity in the usual manner. The rubbers were not attached to a surrounding frame, but to brass arms, which proceeding from a socket through which the axis passes, diverged at equal distances from each other towards the periphery of the plate. The machine has a very compact and neat appearance, and its various smaller parts are contrived with much judgment.

In some comparative experiments made with a plate twenty-two inches in diameter, the superiority of three pair of cushions over two pair was very manifest. In the following table the first column expresses the length in inches of the rubbers; the second the length of the spark when two pair of rubbers were used, and the third the length of the spark when three pair of rubbers were on the machine.

| | | |
|---|---|---|
| 6 inches | 12 inches | 18 inches. |
| 7 ,, | 14 ,, | 21 ,, |
| 8 ,, | 16 ,, | 24 ,, |
| 9 ,, | 18 ,, | 27 ,, |
| 10 ,, | 20 ,, | 30 ,, |

*Bib. Univ.* xxiv. 187.

7. *Improvement of the Leyden Jar.*—M. Metzger has also varied the construction of Leyden jars, so as to augment their capacity without increasing their apparent volume. For this purpose having two jars of proper dimensions, he simply places one within the other, so that they shall apply pretty correctly, and thus have a capacity of charge nearly proportional to the whole surface of coating, without increasing the volume of the whole beyond that of the larger jar. Jars made slightly conical would answer well for this purpose.—*Bib. Univ.* 191.

8. *Electricity on Separation of Parts.*—In the water-proof cloths manufactured by M. Mackintosh of Glasgow, where two pieces are cemented together by caoutchouc dissolved in coal tar oil, the adhesion is such that when the two are torn asunder in the dark, there is a bright flash of electric light, similar to that produced by separating plates of mica, by breaking Rupert's drops, or by breaking barley-sugar, or sugar-candy. Upon trying this experiment with different substances, it was found that flashes of light were distinctly produced, by tearing quickly a piece of cotton cloth.—*Edin. Jour.* x. 185.

9. *Electric Light.*—Having a metallic wire covered with silk, form it into a close flat spiral, taking care that the revolutions touch each other. Their number may be arbitrary, more than twenty-four have not been used. The properties of this spiral when it forms part of the voltaic circuit are well known, but pass through it a charge of common electricity, such as may be taken by two square feet of coated surface, moderately charged, and a vivid light, something resembling that of an artificial fire-work, will occur, originating from the centre of the spires. It may be seen very distinctly without darkening the chamber where the experiment is made.

M. Leopold de Nobili, who describes this experiment, considers the phenomenon as perfectly new. If the wire be folded backwards and forwards, so as to form a rectangular surface, then the electric discharge only produces a faint light at each corner, and this he considers as the light produced by the escape of the electricity into the atmosphere; but the light from the spiral is said to be so vivid and distinct, that once seen its dissimilarity from the former must be instantly evident. He has, therefore, called it electromagnetic light, because of its relation to the magnetic state of the spiral, thinks that it might be made continuous if a sufficiently powerful voltaic battery were used, and has little doubt but that the aurora borealis is such a light elicited by the magnetic state of the earth. —*Bib. Univ.* xxv. 38.

10. *Connexion of Phosphorescence with Electricity.*—The sulphate of quina was shewn by M. Callaud d'Annecy some time since to become highly phosphorescent when rubbed at a temperature of 212°. MM. Dumas and Pelletier have ascertained that it becomes highly negatively electrical when rubbed on woollen cloth, and hence were led to the verification of a suspicion they had long entertained that phosphorescence was an electrical phenomenon. About two or three ounces of sulphate of quina were introduced into a glass flask, and heated for half an hour in a water bath at 212° F., it then by friction gave out a sufficiently intense light. The flask was closed by a cork, through which passed a wire pointed at the inner extremity, and terminated by a ball at the external end; on approaching this ball, two or three times to the knob of a voltaic electrometer furnished with its condenser, having taken care to shake the flask before each contact, the leaves became so electrical as to diverge as much as the instrument would admit of, the electricity being constantly positive.

The sulphate of cinchona, which is phosphorescent like the sulphate of quina, though less so, also became electrical in the same manner. Its electricity, though of the same kind, was not so strong as that of the preparation of quina.—*Ann. de Chim.* xxiv. 171.

11. *Phosphorescence of Acetate of Lime.*—Dissolve any quantity of acetate of lime in water, and place it on a sand-heat in a Wedgewood ware dish, evaporate to dryness without disturbing it. When quite dry, let the bulb of a thermometer be rested on the bottom of the dish, and when the temperature has attained 250° F., the lime will be found to adhere very firmly. If light be now excluded, and the acetate be strongly rubbed with a stiff spatula, it will become highly luminous. Mr. N. Mills.—*Ann. Phil. N. S.* vii. 235.

12. *Preparation of Sulphurous Acid Gas.*—M. Berthier has shewn that this gas may be obtained very pure and abundantly, by heating a mixture of twelve or fourteen parts of sublimed sulphur, and a hundred parts of peroxide of manganese in a glass

retort. The residue in the retort is not a sulphuret of manganese, but a protoxide of manganese mixed with a little sulphate of manganese, and sometimes a little sulphur.—*Ann. de Chim.* xxiv. 275.

13. *Preparation of Sulphuretted Hydrogen.*—The experimental researches of M. P. Berthier, into the production and composition of certain metallic sulphurets, have been referred to at length, vol. xv. p. 148. Since then another paper has appeared by the same chemist, and relating to the same subject, from which we extract the following matter. In the preparation of sulphuretted hydrogen, it is usual to act on sulphuret of iron by diluted sulphuric acid, or sulphuret of antimony by strong muriatic acid; but, for various reasons, M. Berthier recommends the following compounds of sulphur as better. Powdered common iron pyrites is to be mixed with half its weight of dry carbonate of soda, and heated red-hot in a crucible; a fluid sulphuret of iron and sodium is obtained, which may be poured out on a stone to cool, and is then a homogeneous deep yellow mass, possessing a lamellar fracture. It absorbs much water, forming with it a black paste, which when acted on by sulphuric or muriatic acid instantly yields abundance of sulphuretted hydrogen; leaving a black sulphuret of iron, which by the application of acid and heat, will yield a second portion of the gas.

Peroxide of manganese mixed with sulphur or charcoal, and heated to bright redness, becomes a protoxide, which treated with sulphuric acid forms a sulphate. This sulphate powdered, mixed with one-sixth of powdered charcoal, and heated to whiteness in a closed crucible, yields a pulverulent sulphuret of manganese, which when acted on by a mixture of one part sulphuric acid, and one part water, gives abundance of sulphuretted hydrogen, and becomes sulphate of manganese again: one hundred parts of the sulphuret yields 38½ parts of sulphuretted hydrogen.

Of all the sulphurets that of calcium appears to be the most proper for this purpose. It produces abundance of the gas, a hundred parts producing 46.8 of sulphuretted hydrogen; the residue on the action of muriatic acid is entirely soluble, and therefore admits of perfect and free action without the application of heat; and it may be obtained in the greatest abundance. Sulphate of lime is to be reduced to an impalpable powder, and then mixed with powdered charcoal in the proportion of 0.15 of the latter, if the sulphate be a hydrate, but if an hydrous 0.20 of charcoal will be necessary. The mixture is to be put into crucibles, and heated to whiteness for an hour or two in a wind furnace. The sulphuret does not act on the crucible, and is obtained in a pulverulent state. If the sulphuret be required in great quantity it may be prepared by mixing the sulphate of lime and charcoal with a sufficient quantity of plaster of Paris, moistening and moulding the whole into bricks, which may be burnt like bricks of clay.—*Ann. de Chim.* xxiv. 271.

14. *Preparation of Saturated Hydro-sulphuret of Potash or Soda.*—The following is M. Berthier's process: mix ten sulphate of potash, ten sulphate of baryta, and five powdered charcoal, or eight dry sulphate of soda, ten sulphate of baryta and five powdered charcoal, and heat them to whiteness in a crucible. Double sulphurets are obtained, which are greyish, half-fused, and easily separated from the crucible; they contain each an atom of the composing sulphurets. Pulverize and introduce them gradually into a flask three-fourths filled with warm water, close it and frequently agitate it; when saturated, the water will contain an atom of the sub-hydrosulphuret of alkali and an atom of sub-hydrosulphuret of baryta. Diluted sulphuric acid is then carefully added to the solution in the flask, by small portions at a time, agitating each time and preserving the flask well closed. In this way the baryta is precipitated, and its sulphuretted hydrogen goes to the alkali; when all the earth has fallen, the fluid is left to become clear, is decanted and tested by solution of salts of lime or magnesia. If a precipitate occurs, a fresh portion of hydrosulphuret of baryta must be added to the liquor and precipitated by sulphuric acid. With a little care, a neutral hydrosulphuret of the alkali is obtained, which contains neither baryta or sulphuric acid, but of the two it is better to have the acid in excess.—*Ann. de Chim.* xxiv. 279.

15. *Preparation of Kermes Mineral.*—According to M. Fabroni, a much finer kermes mineral is obtained by using tartar in place of the alkali employed in the usual process. Three or four parts of tartar should be mixed with one part of powdered sulphuret of antimony, and heated red in a crucible until the cessation of fumes indicates that the tartar is all decomposed; the mass is then to be dissolved in hot water, filtered and left to cool, when abundance of fine kermes will be deposited, of a very deep colour. The abundance of kermes thus obtained does not at all interfere with the quantity and beauty of the golden sulphuret, afterwards obtained by the addition of acid to the mother liquor.—*Ann. de Chim.* xxv. 7.

16. *Action of Sulphur on Iron.*—Col. A. Evans has remarked, that although sulphur has so strong an action on heated wrought iron as immediately to form holes in it, yet it does not at all affect grey cast iron. A plate of wrought iron, 63 of an inch in thickness, heated to whiteness, and held against a roll of sulphur $\frac{6}{10}$ of an inch in diameter, was in fourteen seconds pierced through with perfectly cylindrical hole. Another bar about two inches in

thickness was pierced by the same means in fifteen seconds. Good steel was pierced even more rapidly than the iron, but a piece of grey cast iron, well scaled and heated till nearly in fusion, was not at all affected by the application of sulphur to its surface, not even a mark being left. A crucible was made of this cast iron, and some iron and sulphur put into it; on applying heat the iron and sulphur soon fused together, but the cast iron underwent no change.—*Ann. de Chim.* xxv. 107.

17. *Economical Preparation of pure Oxide of Nickel,* by M. Berthier.—Speiss, or impure nickel, is to be reduced to fine powder and roasted till it gives off no further vapours of arsenic, the heat being at first moderate to prevent fusion, and then increased. Metallic iron in the state of filings or nails is to be added in a quantity which ought previously to be determined, and the whole dissolved in boiling nitro-muriatic acid, so much nitric acid being used that no protoxide of iron remain in the solution; evaporate to dryness and re-dissolve in water, when a large quantity of arseniate of iron will be left. Add to the solutions successive portions of carbonate of soda until a greenish precipitate appears, at which time all the arsenic and iron will be separated, and part of the copper; the rest of the copper may be separated by sulphuretted hydrogen, and the clear solution thus obtained, when boiled with sub-carbonate of soda, yields the carbonate of nickel.

Thus obtained, the carbonate of nickel contains a little cobalt; to separate the latter, the precipitate as obtained above by boiling with sub-carbonate of soda, is to be well washed and diffused whilst moist in water, and a current of chlorine passed into it until in excess: the excess of chlorine is to be allowed to dissipate and the solution filtered; it now contains not the smallest trace of cobalt, that remaining as a hydrated peroxide, with a certain portion of nickel in the same state. If in the mixed carbonate of nickel and cobalt, the latter is in excess; the residue, after the action of the chlorine, is pure hydrate of cobalt, and the solution contains the nickel with a small quantity of cobalt.—*Ann. de Chim.* xxv. 95.

18. *White Copper.*—According to M. Keferstein, a metallic composition resembling silver has been employed under the name of white copper, for a long time at Suhl, in ornamenting fire-arms. M. Brandes, by analysis, found it to be an alloy of copper and nickel. MM. Keferstein and Muller have recently sought out the origin of this substance, and have ascertained that it is found in the scoria of some ancient copper-works, formerly attached to mines now abandoned. The white copper, which had formerly been rejected as useless, is now obtained by fusion, for the purpose above stated.—*Ann. de Chim.* xxiv. 234.

19. *Prussian Blue.*—Mr. Badnall, of Leek, has taken out a patent for improvements in dyeing with Prussian blue. The improvement consists in preparing the Prussian blue, by mixing it in fine powder with strong muriatic acid, and stirring it until the whole becomes a smooth homogeneous mass of a semi-gelatinous consistence. We notice it here merely to remark on the circumstance that an agent in which Prussian blue is insoluble, should be found useful in enabling it to combine with silk, cotton, wool, *&c.* The pure ferroprussiate of iron is soluble in water, but the addition of a small portion of muriatic acid immediately precipitates it; wash away the acid by pure water, and the pigment becomes soluble again; re-acidify, and it re-precipitates.

20. *Crystallization of the Sub-carbonate of Potash.*—Il Dot. M. Fabroni describes the following process for the crystallization of this salt. Make a solution of pearlash in water, and evaporate it until of specific gravity 1.57. Allow it to cool, when all extraneous salts will be deposited; separate the fluid and again concentrate it until of specific grav. above 1.6. The fluid will now be of a light green colour, and strong alkaline odour; place it in deep vessels, as glass jars for instance, and the sub-carbonate will soon crystallize in long rhomboidal white laminæ, situated vertically and parallel to each other; one extremity will touch the bottom of the vessel, and the other be attached to a saline crust on the surface of the liquid. When cold, the mother liquor will be found of specific grav. 1.6, but if further concentrated and again cooled, more crystals will be obtained; and this may be continued until the whole has been crystallized.—*Gior. di Fisica,* vi. 451.

21. *Composition of Ancient Ruby Glass.*—Mr. Cooper, on analyzing a portion of this glass, sent to him by Mr. C. Muss, found it to contain silex, oxides of copper, iron, and silver, and lime. He considers the oxides of copper and silver as the colouring matter, but from the coloured portion being a film not more than $\frac{1}{200}$ of an inch in thickness, upon the surface of the glass, it was impossible to ascertain their proportions. Iron existed abundantly in the uncoloured portion of the glass. Mr. Cooper thinks the alkali used as a flux for the siliceous matter is soda.—*Ann. Phil.* N. S. vii. 106.

22. *Detection of Arsenic in cases of Poisoning.*—Mr. Phillips, in a very excellent practical paper on the methods of employing the various tests proposed for the detecting the presence of arsenic, has very much facilitated their use in certain cases, by pointing out that where the arsenic is mingled with a complicated mixture of animal and other substances, as when its presence is to be ascertained in fluids from the stomach, animal charcoal may be very advantageously employed as a preparatory agent. Some coloured

liquor arsenicalis on being boiled for a few minutes with ivory black was rendered so colourless that any of the tests for arsenic could be readily applied. The experiment was repeated, substituting for the colouring liquor, port wine, gravy-soup, and a strong infusion of onions, and in all these cases a solution was obtained sufficiently colourless for the application of the most delicate tests. This agent is not liable to any mistake from the presence of phosphates, for water or wine boiled in it alone separated nothing except, in one or two cases, a small portion of a muriate; to avoid the interference of this substance, the ivory black may be washed with boiling distilled water until the washings do not affect nitrate of silver; but good ivory black does not require this treatment.

With regard also to the application of sulphate of copper as a test of arsenic, Mr. Phillips recommends a precaution which has not heretofore been thought of. Sulphate of copper yields a green precipitate when added to potash, white arsenic being present; but if the sulphate contains any peroxide of iron, it may yield a green precipitate with the alkali, the arsenic not being present. Mr. P. has remarked that the arsenic may be added after as well as before the precipitate is formed, for the blue precipitate occasioned by the potash in pure sulphate of copper becomes green when the white arsenic is added. Add solution of potash first, therefore, to the sulphate of copper, and obtain the fine blue precipitate; to a part of this add the suspected solution, and if arsenious acid be present it will convert the blue precipitate into a green one.—*Ann. Phil.* N. S. vii. 30.

In reference to the reduction of arsenic to the metallic state, as a test of its presence, Dr. Trail thinks the general opinion of the large quantity required is unfounded, and easily succeeds in obtaining this evidence from $\frac{1}{10}$ of a grain of white arsenic. The tube is to be 2½ inches long, 0. 4 inch. wide, and closed at one end. The substance thought to be arsenic should be mixed with thrice its weight of black flux, or sub-carb. soda mixed with charcoal powder, introduced into the tube, and a little charcoal powder put over it; the upper part of the tube must be cleaned, and the mouth closed by a piece of paper. The flame of a spirit-lamp will in about two minutes produce a shining metallic crust on the upper side of the tube; when cold shake out the loose materials, scrape off the metallic crust, which will afford sufficient for six different portions, each of which when projected on a dull red-hot poker will give a white smoke and alliaceous odour. A clean knife held in the smoke will always condense a portion of white powder.—*Ann. Phil.* vii. 132.

23. *On the Detection of Acetate Morphia in cases of Poisoning, by M. J. L. Lassaigne.*—The following are the processes recommended. If the acetate of morphia be suspected in a liquid, it is

to be evaporated by a moderate heat, and the residue digested in alcohol, which will dissolve the acetate as well as ozmazome and some salts. The alcoholic solution evaporated, and the residue dissolved in water, will cause the separation of a portion of fatty matter. The last solution is to be evaporated spontaneously, and if it contains acetate of morphia, that substance will crystallize in diverging needles of a yellow colour, and known by, 1, their bitter taste; 2, their decomposition by ammonia; 3, the liberation of acetic acid by strong sulphuric acid; 4, the red colour developed by nitric acid. If the salt be in such small quantity that the ozmazome prevents its crystallization, nitric acid will detect it by the colour produced.

If it is suspected to exist in a solid mixture or substance, it is to be boiled with water for about ten minutes, and then treated as above. If the accompanying substances are alkaline, a small quantity of acetic acid must be added, to form an acetate with the morphia.

By these methods M. Lassaigne has detected the acetate of morphia; 1, in the substances vomited by animals to which it had been given; 2, in the stomach of a cat who died on taking five grains of it; 3, in the liquid from the thorax of a dog which died ten minutes after the injection of fourteen grains of the substance; 4, in the small intestines of a cat which died ten hours after the injection of eighteen grains of the substance into that canal; 5, in the duodenum of a dog which died four hours and a half after the injection of eighteen grains into that part.

It was found also in the blood from the jugular vein of a horse, opposite to that by which thirty grains of the acetate had been injected ten minutes before; but five hours after the injection none could be found, indicating that where the animal could support the poison it was gradually destroyed or expelled. A grain of the salt mixed with six ounces and a half of ox blood, was easily found again after several hours.

Lest the orange colour produced by nitric acid should be due to the presence of an animal substance, M. Lassaigne endeavoured to avoid the presence of any such matter, and found the following process perfect in this respect. A solution of sub-acetate of lead is added to the aqueous solution of the alcoholic extract suspected to contain acetate of morphia, all the colouring and azoted matters are immediately precipitated, and there remains in solution only certain salts with the acetate of morphia, and a slight excess of acetate of lead, which latter may be decomposed by a few bubbles of sulphuretted hydrogen. The solution should then be evaporated in vacuo over sulphuric acid, and if it contains acetate of morphia, that substance will soon crystallize, its base may be separated, and the colour by nitric acid is no longer equivocal.

The conclusions appended to this mémoire, are, 1, that it i possible in many cases of poisoning by acetate of morphia, to discover sensible traces of this vegetable poison; 2, that the substances vomited shortly after taking the poison into the stomach contain ponderable quantities of it; 3, that it is always in those viscera into which it has been introduced, that its remains may be detected; 4, that all attempts as yet made to discover it in the blood of the dead animals have been fruitless.—*Ann. de Chim.* xxv. 102.

24. *Test for Morphium*—M. Dublane a druggist of Paris, states that he finds the tincture of nutgalls a very sensible test of the presence of morphium in fluids, whether it exist free or in combination with acetic or sulphuric acid.—*Ann. de Chim.* xxv. 92.

25. *Process for obtaining Strychnia, by M. Ferrari.*—Boil three pounds of bruised nux vomica for two hours in thirty pints of water acidified by six ounces of muriatic acid, and pass the liquid through a cloth or sieve: the residue should be thrice boiled again for the same time, and in equal quantities of acid and water. To the united cold infusions add lime slowly whilst mixing, until in considerable excess; after two or three days decant the liquid, collect the paste on a filter, dry and powder it. The decanted liquor should be rather more than neutralized by muriatic acid, evaporated until reduced to a few pounds, when cold precipitated by lime, allowed to stand, decanted, and the residue when dry, powdered and added to the former.

Sulphuric acid may be substituted for muriatic acid in the process, but the three pounds of nux vomica will require only three ounces of this acid in twenty pints of water, and the boiling should continue one hour only. Afterwards, the liquid rendered acid is to be concentrated until like syrup, being agitated during the evaporation if any deposit appears. To the cold liquor powdered lime is to be added, and the process goes on as before.

The mixed precipitate of lime and strychnia obtained by either of the above methods is to be heated in a water-bath two or three times, with alcohol of specific grav. 0.832, until all the bitter principle is extracted. The united fluids are to be distilled, and this operation finished; there will remain a yellow turbid bitter alkaline fluid, which is to be decanted off and reserved, and beneath it the strychnia soiled by a yellow colouring matter, which will harden upon cooling. This mixture treated with cold alcohol of specific gravity .915 will leave pure strychnia.—*Gior. de Fisica.*

26. *Volatility of Salts of Strychnia.*—Il Sig. Ferrari has remarked that solutions of salts of strychnia slightly acid when exposed to a heat of 212°, so as to be concentrated, then become volatile and the salt evaporates. This property has been remarked in the sul-

phate, nitrate, muriate, and acetate, and is believed to belong to all the salts. It has been remarked by M. Collaud and others, that the sulphate of quina is also volatile, and M. Ferrari, on repeating the experiments with the muriate and nitrate of quina, found it also to happen with them. The solutions on being heated in a tinned copper vessel, gave out vapours which when breathed, were found to be highly bitter. The salts vary in the extent of this property, and it is also affected by the degree of acidity, and of concentration of the solution.—*Gior. de Fisica*, vi. 460.

27. *Acid Tartaro-Sulphate of Potash.*—Il Sig. M. Fabroni says that sulphuric acid being boiled with thrice its weight of water, and cream of tartar in excess, gives a fluid, which after having been evaporated, cooled, and allowed to deposit undecomposed tartar, sulphate of potash, *&c.*, will not furnish any other deposit, and resembles oil in its appearance. When further evaporated to the consistence of syrup, and again cooled, it solidified in a mass composed of imperfect prismatic crystals, and which when dry, had something of the appearance of camphor. It dissolves rapidly in water, but in alcohol yields its tartaric acid, and acid sulphate of potash is left. On analysis it gave seventy-two tartaric acid, and twenty-eight acid sulphate of potash. M. Fabroni thinks that for many uses this salt may be a cheap and effectual substitute for tartaric acid. He considers it as analogous in its nature to the compound of tartar and boracic acid.—*Gior. de Fisica*, vi. 452.

28. *Pyroligneous Ether, or Pyroxilic Spirit.*—A brief description is given* at p. 436, vol. xiv, of this Journal, of a substance obtained by Mr. P. Taylor, first in 1812, and at various times since then, from the distillation of wood. M. Taylor called it pyroligneous ether. Latterly this substance has been re-examined with great care, by MM. Macaire and Marcet, of Geneva, who have called it pyroxilic spirit, in their paper upon it, published in the *Bibliothèque Universelle.* The following is a brief account of their observations. The fluid is transparent, colourless, of a strong ethereal odour slightly resembling that of ants. Its taste is hot and strong, leaving an impression on the tongue like that of essence of mint: its specific gravity .828. It boils at about 150° F. Its slightly acid properties appear to be due to a little acetic acid. It burns away entirely with a perfectly blue flame. Alcohol dissolves it in all proportions, but water separates it again. With water only, it forms a sort of emulsion, which is of considerable permanence. It does not dissolve in oil of turpentine. It dissolves camphor, but not oil of olives either hot or cold. It also dissolves pure potash.

Heated with its volume of sulphuric acid, it distils over unchanged; heated with thrice its volume of sulphuric acid, it blackens, swells, and liberates a small quantity of inflammable gas, which burns with a pale flame, is not condensed by chlorine, and appears to be a mixture of proto-carburetted hydrogen and hydrogen. When distilled with its volume of nitric acid nitrous vapours arise, and an ethereal liquid distils over, which when distilled from oxide of lead, reddens litmus, has an agreeable odour, burns with a strong greyish flame, dissolves in water and alcohol, communicating a sweet mild taste, and in all its properties quite unlike nitric ether. A current of nitrous gas passed into a portion of the pyroligneous fluid effected no change in it. Muriatic acid produced no effect upon it.

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* From the *Phil. Mag.* LX. 315.

A current of chlorine passed into the fluid, made it of a deep yellow colour, but continuing the current a few minutes, the colour on a sudden disappeared, six parts of the fluid had thus increased to six parts and a half, of a colourless transparent liquid, fuming by ammonia, having a poignant odour, and exciting tears. It burnt with a blue flame, producing abundant fumes of muriatic acid, and an odour resembling horseradish. When distilled from litharge, it passed over less acid, but otherwise unchanged. Its specific gravity was 0.889; it was soluble in water and alcohol, communicating a strong taste of horseradish. It precipitated nitrate of silver, and became more acid by exposure to air and light. This compound as well as that produced by the action of nitric acid, appears to be an ether, having particular properties; and these ethers prove that the pyroligneous fluid is in its relation to acids analogous to alcohol.

MM. Macaire and Marcet then prepared some of the pyroacetic spirit described by Chenevix, and instituted comparative experiments on it, and Taylor's fluid. The pyroacetic spirit is lighter than the pyroligneous fluid, being according to Chenevix of specific gravity 0.786. Its taste and smell are different. It burns with a strong white flame, and is quite soluble in oil of turpentine. Sulphuric acid does not trouble or blacken it, but produces a fine yellow red colour, and the fluid remains transparent until heated. Distilled with muriatic acid, a volatile fluid passes, and a black substance remains; distilled on potash the fluid loses its acid odour, and the residue smells like tar. Chlorine passed into the pyroacetic spirit, rendered it of a slight yellow colour. The fluid resulting had a strong suffocating smell, resembling that of the substance obtained by treating the pyroligneous fluid in the same way, but after a time it separated into two portions, the one thick, oily, heavy, and transparent, the other light, and slightly opalescent. The latter burnt with a light blue flame, being an acid residue. It is soluble in water, communicating a hot taste to it, but not like horseradish. The oily fluid burnt with a dense green flame, and the production of much muriatic acid, it was soluble in alcohol, but insoluble in water, at the bottom of which it lay in drops.

Finally, MM. Macaire and Marcet proceeded to analyze these fluids, and this they effected by oxide of copper: one hundred parts of the pyroxilic spirit or pyroligneous ether gave

44.53 of carbon, or 6 atoms.
46.61 oxygen, 4 „
9.16 hydrogen, 7 „

One hundred parts of the pyroacetic spirit of Chenevix gave

55.30 of carbon, or 4 atoms.
36.50 oxygen, 2 „
8.20 hydrogen 3 „

Analyzing alcohol of specific gravity .820 at the same time, one hundredparts gave

48.8 of carbon, or 3 atoms.
39.9 oxygen, 2 „
11.3 hydrogen 5 „

The conclusions of the mémoire are, 1st. That there exist at least two simple vegetable fluids distinct from alcohol, but like that liquid, having the property of forming with acids, particular ethereal spirits; 2nd. That these two fluids which may be distinguished by the names pyroacetic spirit and pyroxilic spirit, differ from each other, both in their properties and composition.—*Bib. Univ.* xxiv. 126.

We suspect some mistake in the printing of the figures of the analysis, for there is no accordance in the estimation of the weight of the atoms deduced from them. If the weights expressed be divided by the number of atoms assigned, and the whole be reduced to the atom of hydrogen, as unity it gives the weight of an atom of carbon 5.67 by the first analysis, 5.06 by the second, and 7.2 by the third, and the weight of an atom of oxygen as 8.9 by the first analysis, 6.67 by the second, and 8.82 by the third.—*Ed.*

29. *Cafeine.*—Cafeine is a crystallizable principle discovered in 1821, in coffee, by M. Robiquet, whilst searching in it for quina. MM. Pelletier and Caventour obtained this substance at the same time, but did not complete their researches. M. Robiquet read a mémoire on this subject to the Société de Pharmacie of Paris, which has not been published. It is, however, known to be a new principle, white, crystalline, volatile, and slightly solubl—e. *Dict. de Med.*

Its composition is very remarkable, for according to MM. Dumas and Pelletier, it consists of

| | |
|---|---|
| Carbon . . . | 46.51 |
| Nitrogen . . | 21.54 |
| Hydrogen . . | 4.81 |
| Oxygen . . . | 27.14 |
| | 100. |

The quantity of nitrogen in it surpasses that of most vegetable substances.—*Ann. de Chim.* xxiv. 183.

30. *Conversion of Gallic Acid into Ulmin*—The following statement is by M. Doebereiner. On dissolving a determinate quantity of gallic acid in ammonia, and placing the solution in contact with oxygen, it absorbed sufficient to convert all the hydrogen of the gallic acid into water. In this way the acid became converted into ulmin, which is composed of

| | | |
|---|---|---|
| 1 atom . . . | 12 | carbon |
| 1 „ . . . | 1 | hydrogen |
| 2 „ . . . | 16 | oxygen |

and may be represented as a combination of two volumes of gaseous oxide of carbon, and one volume of vapour of water.—*Ann. de Chim.* xxiv. 353.

There is some mistake in the above statement of the composition, but the fact is very curious—*Ed.*

31. *An Account of an Electrical Arrangement produced with different Charcoals, and one conducting Fluid, communicated by Mr. T. Griffiths.*—In the course of some experiments on charcoal, the results of which are given in a late number of the Journal of Science, two specimens were obtained differing remarkably in mechanical texture, and electrical conducting power, but more especially in the former. One of them being soft and porous, absorbing water with great avidity; the other hard and compact, absorbing it with comparative slowness. I was induced from the observation of this fact, to try if it would be possible to form an electrical arrangement of several such pieces made into arcs and plunged into glasses of water; supposing that the absorption of that fluid taking place more rapidly in the one than the other, might at the time develope electricity.

An apparatus was accordingly constructed, consisting of several pieces of the two charcoals united by a wire into the form of an arc, and dipping at their extremities into glasses of pure water. Upon connecting its opposite ends with the tongue, a perceptible taste was experienced similar to that produced by a very feeble galvanic combination; the limbs of a newly-killed frog underwent evident convulsions when made part of the circuit.

In order to remove any source of fallacy that might have attended the employment of a metallic wire to connect the charcoals, another apparatus was made in which they were united into arcs by cotton or silk threads, and upon examining it by the tongue and the limbs of a frog, the effects were similar to those before produced in the first form of experiment.

Upon making a tube filled with water part of the circuit, a decomposition was expected, but none took place, although the experiment continued several hours, nor would it revive copper from its solution in sulphuric, or acetic acids, so that its respective poles have not been distinguished.

In all experiments made with this apparatus, the employment of metals was carefully excluded, so that their contact with the charcoal should not give incorrect results.

If when the limbs of a frog are undergoing convulsions, one of the arcs be removed from the circuit, they instantly cease, but return again, upon its being replaced; and it is a curious fact that the effect on the limbs is decidedly most powerful, when the nerve is in contact with the rapidly absorbing surface; if the opposite arrangement be adopted making the muscle in contact with it, the effect is greatly diminished, or altogether ceases.

That the activity of the apparatus is dependent upon the absorption of water, is proved by its cessation in about twenty-four hours, the charcoals becoming saturated with water; but by heating them red hot it is expelled, and upon again arranging them in the manner mentioned, they will be found to regain their former activity.

A solution of common salt, being employed as the fluid, augments the effect of the apparatus, it being a better conductor; but if it is wished to heat the charcoals for another experiment, they should be soaked in water to dissolve the salt, which would otherwise fuse, and fill up the pores. The woods from which the charcoals are obtained, are known by the names of Botany Bay, and King wood. The former should be chosen full of dark streaks, which open when exposed to heat, and give the resulting charcoal a great degree of porosity. In selecting the other wood, no very particular attention is required, it generally producing a charcoal of pretty uniform density.

## III. Natural History.

1. *Vegetation at different Heights.*—The following is a table constructed by Mr. Bakewell, of the height at which various trees and shrubs grow in the Vallois and Savoy. The extreme height

implies situations open to the south and west, and sheltered from the north-east wind, the height varying very much according to the aspect in an alpine country. The heights are expressed in English feet above the level of the sea, lat. 45° 30′ to 46° 30′.

| | |
|---|---|
| Vines . . . . | 2380 |
| Maize . . . . | 2772 |
| Oak . . . . | 3518 |
| Walnut tree . . . | 3620 |
| Yew tree . . . . | 3740 |
| Barley . . . . | 4180 |
| Cherry tree . . | 4270 |
| Potatoes . . . . | 4450 |
| Nut tree . . . . | 4500 |
| Beech tree . . . | 4800 |
| Mountain Maple . . | 5100 |
| Silver Birch . . . | 5500 |
| Larch . . . . | 6000 |
| Fir le sapin . . . | 6300 |
| Pinus cembra . . . | 6600 |
| Rhododendron . . . | 7400 |

The line of trees reaches the height of 6700, the line of shrubs 8500. Some plants on a granitic soil grew at 10,600, above which are a few lichens, but vegetation ceases at 11,000. In the Garden of the Inn, kept in summer at the Schwarrenbach, on the passage of the Gemmi, carrots, spinach, and onions, are cultivated at the height of 6,900 feet.

In the southern part of Savoy, the height at which pines will grow is about 2,600 feet, but near this elevation the crops failed in the cold summer of 1821.—*Bakewell's Travels.*

2. *Irritability of Plants.*—Whilst experimenting on the irritability of certain plants,as the sensitive plant for instance, Dr. Meyer had occasion to observe, that of those substances which acted by being absorbed into the plant, the most volatile were also the most powerful, although not the most destructive. When the extreme leaflets of a branch were moistened with naphtha or essential oil, the influence gradually extended itself to the neighbouring leaflets, and even to the other leaves of branches. Their recovery was in the inverse order of their depression. Another observation by the same author on these plants is, that when affected by a trembling motion the leaflets close, but if the motion be continued or some hours they will again open.—*Bib. Univ.* xxv. 53.

3. *Notice of an undescribed Larva which attacks and devours Snails.*—The account of this larva was read before the Society of Natural History of Geneva, by Count Milzinsky. As far as the

Count could ascertain by reference to books, and by inquiry amongst those best acquainted with insects, it had never been described. The insect was five or six inches long, and two or three broad; its colour yellow; it was furnished with two long bifurcated mandibles; had at the upper part two brown antennæ, each composed of two articulations, and supported on a white membraneous projection; beneath the mandibles were four feelers, two of them in constant motion. The body is divided into twelve rings, the three anterior have each two strong feet, and but little hair; the following eight have each two false feet, and on each side two tufts of hair; the twelfth has two large terminal tufts of hair, which serve as a case to a sort of cartilaginous tail which the animal moves at pleasure and uses as a sort of supplementary foot: it is hollowed at the extremity, and covered with a viscid humour. Between the lines formed by the tufts are two ranges of projecting glandular black points, considered by M. Milzinsky as trachia.

The larva is excessively voracious, attacking and apparently feeding entirely on snails. On meeting with a snail, if the animal be out of its shell, the larva takes a position on the shell and does not attack the snail until it has entirely entered its habitation; the larva then approaches the right side of the snail and forcibly plunges his head into it, helping itself powerfully by the use of the hind foot. The snail gives evidence of suffering, and endeavours to withdraw into and go out of its shell, moving much about, but in a short time it ceases its motion and dies. The means by which the larva produced so quick a death to the snail could not be ascertained, for all passed so much within the shell as to be withdrawn from observation. During the time that the larva remains in the body of the snail, either alive or dead, only the terminal tufts of hair are seen without. The larva will sometimes in this manner attack and destroy three snails in one day.

These insects are generally found in dry ditches or by hedges. If a snail's shell be observed that has recently fallen, and the first spire be broken, one of these animals will almost certainly be found within. They vary in size, and are proportionate to the snails in which they are found. A small larva, on devouring a snail, grows considerably, changes its skin, and then searches for a larger snail. When it has attained its final size it attacks its last snail, rejecting with force, towards the middle of its operation, a semi-liquid decomposing matter; and by the time it has eaten or emptied all the contents of the shell (the shell remaining clean) it has become large, white and shining; it then remains inactive for a variable portion of time, afterwards changes its skin, but in a manner different to the previous changes, and becomes a chrysalis. In this state it remains awhile, and preserves its tufts, but less apparent than in the former state. The chrysalis remains at the bottom of the shell for two or three months, and then on a sudden becomes white; shortly the spots and colours of the skin appear, and the insect ultimately passes into its perfect state, when it deposits its eggs. All these changes take place within the shell, and it is difficult to ascertain them without disturbing the animal and deranging the results.

Drawings of the animal having been shewn to MM. La Freille and Audouin, they are inclined to believe that the insect in its perfect state is not merely a new genus, but a particular family, which they would place in the order of Thysanours, or in that of parasites.—*Bib. Univ.* xxiv. 137.

4. *Hatching Fish.*—The Chinese have a method of hatching the spawn of fish, and thus protecting it from those accidents which ordinarily destroy so large a portion of it. The fishermen collect with care on the margin and surface of waters all those gelatinous masses which contain the spawn of fish; after they have found a sufficient quantity they fill with it the shell of a fresh hen's egg, which they have previously emptied, stop up the hole, and put it under a sitting fowl. At the expiration of a certain number of days they break the shell in water warmed by the sun, the young fry are presently hatched, and are kept in pure fresh water till they are large enough to be thrown into the pond with the old fish. The sale of spawn for this purpose forms an important branch of trade in China.

5. *Natural Changes in Carrara Marble.*—Carrara marble presents, according to M. Ripetti, an instance of chemical changes in the colouring principles without any alteration in the carbonate of lime. The marble of Carrara does not always possess that brilliant whiteness for which it is so famed; it is for the most part of a greyish tint, and is of its utmost whiteness only in certain parts where veins have been formed, or else spots of oxide, sulphate, or sulphuret of iron. Some of these stains are old and fixed, but others seem to be of recent formation and are removed by water running over them, so that in a short time the marble becomes as white as snow. The workmen express this effect by saying, "The marble cleanses itself." Whole masses seem to change by a chemical process, and in support of this opinion, it has been observed that the marble of the ancient excavation of St. Silvestro, which was formerly of no value, has now become excessively white: and that in general the different species of Carrara marble vary with time, and become more and more pure.—*Gior. de Fisica.*

6. *Note on the existence of a Nitrate and a Salt of Potash in Cheltenham Water,* by M. Faraday, *&c.*—Having undertaken at the request of Dr. Creaser an examination of some water from Chelten-

ham, I had occasion to remark in it the existence of two substances not before observed in waters from that place; and though of no importance in a medicinal point of view, yet as relates to the sources from whence the waters obtain their impregnations, and to the illustration they afford of the use of two tests suggested by Dr. Wollaston but not very frequently, I believe, in the hands of chemists, they may I think possess interest; one of these substances is nitric acid, and the other potash.

The source from which the water was obtained is called, I believe, the Orchard Well. It had been some time in disuse, but has more lately been cleaned out and deepened, and is now about fifty-six feet to the bottom. The solid contents of a pint of this water examined in London were,

| | |
|---|---|
| Carbonate of lime . . . . . . | 1·6 gr. |
| Sulphate of lime . . . . . . | 14·5 |
| Sulphate of magnesia . . . . . | 12·4 |
| Sulphate of soda . . . . . . | 3·7 |
| Muriate of soda . . . . . . | 97·0 |
| | 129·2 |

Besides which, the water contained a portion of carbonic acid; and a small quantity of peroxide of iron had settled to the bottom of the bottle.

On adding sulphuric acid to a portion of this water in quantity abundantly sufficient to decompose all the salts subject to its action, and boiling such acidulated water in a Florence flask, with a leaf of gold for half an hour or an hour, the gold either in part or entirely disappeared, and a solution was obtained which when tested by proto-muriate of tin, gave a deep purple tint. Hence the presence of nitric acid, originally, in the water was inferred, and that no mistake might occur, a solution was made in pure water of all the salts except the nitrate found in the water, boiled with some of the same sulphuric acid, and tested by the same muriate of tin; but in this case no colour was afforded, or any gold dissolved.

The potash was ascertained to be present by evaporating a quantity of the water until reduced to a small portion, filtering it and then adding muriate of platina in solution. Three pints of water, evaporated until about one ounce of fluid remained, gave an abundant precipitate of the triple salt of potash and platina. In cases where small quantities of the water was tried, it was necessary to let the liquid stand an hour or two after applying the muriate of platina, but the triple salt always ultimately appeared.

Two pints of the water, evaporated to dryness in a silver crucible, gave on re-solution of the residuum a decided though very minute trace of silica.

7. *Iodine in Mineral Waters, &c.*—At p. 168 of the last volume of the Journal, was noticed the presence of iodine in the waters of Sales in Piedmont. It was discovered in them by M. Angelina. It appears that since then M. Krüger of Rostock, has found iodine in the mother liquor of the saline springs of Sülzer in Mecklenburgh-Schwerin, and M. Fuschs has found the same substance in the mother water of the Sal Gem of Hall in the Tyrol. It appears, however, that as yet the iodine has not really been separated from their mother liquors, but its presence has been ascertained by the blue colour given to starch dissolved in nitric acid, and there appears to be no doubts in the minds of the experimenters on the reality of its presence.—*Gior. de Fisica.*

8. *New Vesuvian Minerals.*—MM. Monticelli and Covelli mention the following minerals as having been sent forth to the surface of the earth during the eruption of Vesuvius in October, 1822. 1. Two small pieces of true lapis lazula found in the red sand sent forth on the 24th of October. 2. Several varieties of quartz, resinous quartz, and its passages into a lava composed of amphigene and pyroxene. 3. White and green phosphate of lime in fine hexaëdral prisms and acicular crystals. 4. Perfect cubes of melilite, much larger than those of Capo di Bove. The two latter species were found in a current on the sides of Monte Somma above Pollena. 5. Gehlenite resembling that of Tassa. 6. Specula iron in brilliant plates above an inch wide. 7. Oxide of iron in octoëdrons, above half an inch in diameter; the same also in mammelated or fused masses. 8. Antimonial iron. 9. Glass of antimony apparently containing a small quantity of osmium.—*Bib. Univ.* xxv. 42.

9. *Products of Combustion of certain Coal Strata.*—In the neighbourhood of Aubin (Aveyron,) there exist certain coal strata, some of which are worked, and others are burning, having been on fire for thirty or forty years. It has been remarked as singular that no muriatic acid or ammonia occur in the products of this combustion: much sulphurous acid escapes, and various portions of sublimed sulphur, and acid aluminous efflorescences have been collected; but on chemically examining these and the other products obtained, neither muriatic acid nor ammonia have been observed. The coal nevertheless contains abundance of azote, and on distillation affords carbonate of ammonia.

10. *Advancement of the Ground.*—The inhabitants of the village of Hayotte, in the parish of Champlain, Canada, were alarmed on the 28th of August, 1823, by the motion of a large tract of land, containing a superficies of 207. arpents, It moved five or six arpents, (about three hundred and sixty yards) from the water's

edge, and precipitated into the river Champlain, overwhelming in its progress barns, houses, trees, *&c.* The river was dammed up for a distance of twenty-six arpents in an instant with an awful sound, and a dense vapour, as of pitch and sulphur. Various causes have been assigned for this phenomenon, of which the most probable, is the insinuation of water between the strata.—*Phil. Mag.* lxii. 470.

11. *Existence of Free Muriatic Acid in the Stomach.*—The following are the proofs of the existence of free muriatic acid which Dr. Prout has laid before the Royal Society. The contents of a stomach having been digested in distilled water, the solution obtained was divided into four equal parts. One of these evaporated to dryness, burnt and examined in the usual way, gave the quantity of muriatic acid in combination with fixed bases. A second being previously saturated with an alkali, was treated in a similar way, and gave the whole quantity of muriatic acid in the stomach. A third carefully neutralized with a known solution of alkali, gave the quantity of free acid. The fourth was reserved for any required experiment. In this way Dr. Prout ascertained that the unsaturated muriatic acid in the stomach was always considerable, and in one case twenty ounces of a fluid from a very deranged stomach, afforded him above half a drachm of muriatic acid of specific gravity 1.160.

12. *Use of Sulphate of Copper in Croup.*—Dr. H. Hoffman recommends the sulphate of copper as an excellent remedy in croup, especially after blood-letting. In slight cases he begins with giving from a quarter to half a grain every two hours; in those cases, however, where there is also laryngites, or bronchites, three, four, or more grains are administered, so as to excite instant vomiting; by so doing, the Dr. thinks that not only is the lymph expelled from the trachea, but also that the further secretion of it is prevented, so that the patient is very much relieved, and soon cured. After copious vomiting has been produced, the medicine is to be given in small doses, in conjunction with digitalis. In support of the utility of the above practice, Dr. H. affirms that he has employed it with the greatest success during a period of ten years, in a great number of children affected with croup, without ever having lost a patient in that time, notwithstanding the disease was often at its height when he was first called in.—*Med. Rep.* N.S. i. 85.

13. *On Sand-drigs or Fulgorites, by* MM. Fiedler and Hagen.—The ensuing observations have been selected from the account given in the ***Bib.*** *Universelle,* respecting these natural sand-tubes, by MM. Fiedler and Hagen. The latter person was particularly well circumstanced in ascertaining the cause of their formation. The

Report of Dr. Fiedler states, that being anxious to investigate the circumstances and formation of these tubes in the sandy districts of Austria, he passed over those parts from Vienna towards Hungary, and from thence to Stampfen, in search of them, and was ultimately fortunate enough to find one on the most elevated part of one of the hills, in the neighbourhood of Zankendorf, about a league north of Malaczka.

This tube was half an inch (of Leipzic) at the longest diameter at the upper part. On carefully removing the sand round it, commencing at some distance, it was found that at the depth of two ells a thin bed of quartz, in large grains, occurred, and immediately beneath that, a yellow plastic clay. The sand was now removed round the tube, and it was found that although above it formed an angle of about 80° with the horizon, yet it soon became vertical, and continued so to its lower extremity. It was probably at first some feet longer, but had been destroyed by the wind, for fragments were found lying about, which, however, could not be adapted to the upper end of the tube. At six inches from its upper extremity a small branch, about 4¼ inches, passed off from it, and 32 inches lower the trunk was divided into two branches. The N. E. branch was 7½ inches long and terminated on the clay by a lengthened swelling, hollow within, its surface being composed of fused siliceous sand. In many places the course of the electric fluid could be perceived on the clay by the various red tints it had produced, which penetrated the clay to a depth of eight inches. The fusion appeared to have ceased when the electricity gained the clay. The S.E. branch was 1½ inches longer than the other; before arriving at the clay it passed close round one side of a piece of quartz, an inch in diameter, and was fused to it. It terminated on the clay just as the other branch did, the extremities being removed about 2½ inches from each other.

In many places the tube was contracted to a small diameter, in which places long bulbs had been formed, but the tube itself was hollow within to a considerable extent. It resembled exactly the tubes of the same kind found in the lands of Senner, and like them was surrounded with red sand*. Just beneath the point of separation lay a quartz pebble, but as it was entirely surrounded with sand, it probably had no influence in producing the division.

The following is from the account given by M. Hagen, of a tube formed by lightning, and which he examined soon after its formation. It occurred at the village of Rauschen, on the shores of the Baltic, in the province of Sarnlande. A storm occurred on July 17, 1823, which, about evening, approached the village; near seven o'clock the clouds descended towards a young birch-tree, about twelve feet high, and the lightning descended along its trunk and penetrated the soil at its foot. A man, who was standing at the door of a building, about fifty steps off, saw a juniper bush still burning under the tree, but it was soon extinguished by the rain. The neighbours immediately came together at the tree; they found two narrow deep holes in the earth, and affirmed that one of them was hot to the touch.

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* The sand in the neighbourhood became red by heat.

On examining the tree and place two or three days afterwards, slight traces of the passage of the lightning was found on the tree, and the juniper bushes and herbs in the neighbourhood of the tree, were generally charred. The holes in the earth, however, presented no signs of combustion, not even on the small roots which appeared on their inner surface. The soil was a coarse yellow sand, reposing, at the depth of two feet, on a bed of vegetable earth. On removing the sand, *&c.*, it was observed that oneof the holes did not descend more than a foot, and offered nothing remarkable, but a little lower down, the commencement of a vitreous tube was found; the tube could not be removed whole, because of its fragility, but the fragments were collected, and it was found to have penetrated even into the vegetable earth, where, though many grains of sand had been agglutinated, they had not formed a regular tube. The fragments were covered with a black matter. The other aperture, which had been fonnd hot after the descent of the lightning, did not seem to be accompanied by, or terminate in any vitreous tube.

Some of the fragments withdrawn were three inches long, and all were distinguished from similar tubes or fragments from other places, by their thinness and fragility; they were scarcely as thick as paper, and were semi-transparent. The surrounding sand appeared blackened here and there; the interior of the tube was bright and shining from a thin coat of flux. It was of a pearl gray colour and beset with black points. The tubes were flattened, and extended on opposite sides in a zigzag direction. The sides of the tubes, in some parts, almost came together, but no branches were sent off, except where it had penetrated the vegetable earth, and at that part it became almost filamentous. The fragments together formed a length of above 21½ inches. On examination, the black powder appeared to be carbonaceous, for it resisted the action of acids, but disappeared before the blow-pipe.—*Bib. Univ.* xxiv. 156.

## *On the Recurrence of the Smallest Light of the Variable Star Algol.*

*by* W. M. Moseley, Esq.

*Blackman Street, March* 22, 1824.

My dear Sir,

If you can find a space for the accompanying communication, you will render a service to practical astronomy.

I remain, dear Sir,

Yours, very truly,

J. SOUTH.

*To W. T. Brande, Esq.*

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*Winterdyse House, Bewdley,*
*March*, 20, 1824.

" Dear Sir,

" In compliance with your suggestion, I transmit you a table of the recurrence of the smallest light of Algol; the most remarkable of all the variable stars; Professor Wurm has inserted in Bode's Jahrbuch for 1822, p. 119, a table of this periodical change during the years 1820, 1821, and 1822, and what I now send, is a continuation for the ensuing summer, calculated from the data given in the professor's introductory dissertation. He informs us, that he had verified the period of the change of light, by comparing a recent observation of his own, on September, 23rd, 1813, with Mr. Goodrich's, of earliest date, or January 31st, 1783, after 4540 revolutions had occurred in the interval. The complete change occupies eight hours, or eight hours forty minutes; but this period is very difficult to ascertain accurately. The star when smallest, appears of the fourth magnitude; but when brightest, of the second. It seems, however, from Mr. Goodrich's observations, that when at a maximum, its brightness is different at different times; and Mr. Pigot remarks, that it sometimes is *more luminous* than α Persei. It is convenient to place a bar in the eye-piece of the telescope, of such diameter as just to cover a star of the fourth magnitude; the enlargement will then be more distinctly perceived in its progress; but it will require considerable experience to mark the extremes satisfactorily. The table is reduced to *mean* time at Greenwich. The recurrence of the most diminished light is given to the nearest minute; and on those days only when the star may be distinctly seen at the expected hour.

1824.

| | | H. M. |
|---|---|---|
| April | 14 | 10.25 |
| | 17 | 7.14 |
| May | 7 | 8.57 |
| | 24 | 13.51 |
| June | 13 | 15.34 |
| | 16 | 12.23 |
| | 19 | 9.12 |
| July | 6 | 14. 6 |
| | 9 | 10.54 |
| | 26 | 15.48 |
| | 29 | 12.37 |
| August | 1 | 9.26 |
| | 17 | 14.20 |
| | 20 | 11. 9 |
| | 23 | 7.58 |
| Sept. | 7 | 16. 3 |
| | 10 | 12.52 |
| | 13 | 9.41 |
| | 16 | 6.30 |

"I beg you will dispose of this account as you think proper, and

I hope it will be found to correspond with the phenomenon. It is a tedious process to go through, but I hope I have made no mistake. I remain, dear Sir,

Yours, very respectfully,

W. M. Moseley.

" *To J. South, Esq.*

### Note.

" To preclude all possibility of error in identifying the star, its right ascension is 2h 57′ and declination 40° 16′ N."

## Art. XV.—METEOROLOGICAL DIARY for the Months of December, 1823, January and February, 1824, kept at Earl Spencer's Seat at Althorp, in Northamptonshire.

The Thermometer hangs in a North-eastern Aspect, about five feet from the ground, and a foot from the wall.

| *For December, 1823.* | | | | | | | | *For January, 1824.* | | | | | | | | *For February, 1824.* | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | Thermometer. | | Barometer. | | Wind. | | | | Thermometer. | | Barometer. | | Wind. | | | | Thermometer. | | Barometer. | | Wind. | |
| | | Low | High | Morn. | Eve. | Morn. | Eve. | | | Low | High | Morn. | Eve. | Morn. | Eve. | | | Low | High | Morn. | Eve. | Morn. | Eve. |
| Monday | 1 | 47 | 49 | 29.42 | 29.61 | WbS | SW | Thursday | 1 | 38 | 46 | 29.14 | 29.05 | W | SW | Sunday | 1 | 28 | 42 | 29.79 | 29.71 | E | SE |
| Tuesday | 2 | 39 | 48 | 29.38 | 29.30 | SE | SSW | Friday | 2 | 44 | 45 | 29.20 | 29.60 | WbS | WNW | Monday | 2 | 30 | 41 | 29.82 | 29.90 | SE | SE |
| Wednesday | 3 | 37 | 43 | 29.30 | 29.37 | SW | SW | Saturday | 3 | 36 | 43 | 30.10 | 30.23 | W | NW | Tuesday | 3 | 34 | 43 | 29.94 | 29.80 | SE | SbW |
| Thursday | 4 | 40 | 44 | 29.05 | 29.30 | W | W | Sunday | 4 | 31.5 | 44 | 30.38 | 30.38 | NW | NW | Wednesday | 4 | 38 | 47 | 29.53 | 29.57 | SW | SW |
| Friday | 5 | 35 | 42 | 29.56 | 29.60 | W | W | Monday | 5 | 39 | 42 | 30.38 | 30.32 | W | SW | Thursday | 5 | 33 | 41 | 29.62 | 29.70 | W | W |
| Saturday | 6 | 33 | 42 | 29.41 | 29.76 | NE | NNW | Tuesday | 6 | 36 | 40 | 30.13 | 30.11 | SW | WbN | Friday | 6 | 33 | 42 | 29.83 | 29.90 | W | SW |
| Sunday | 7 | 29 | 36 | 30.30 | 30.30 | WbN. | SW | Wednesday | 7 | 29 | 40 | 30.25 | 30.24 | NNW | NNW | Saturday | 7 | 39 | 46 | 29.90 | 29.90 | SW | SW |
| Monday | 8 | 31 | 43 | 30.26 | 30.13 | WSW | WbS | Thursday | 8 | 30.5 | 39 | 30.20 | 30.13 | SbE | SW | Sunday | 8 | 41 | 54 | 29.98 | 30.10 | SW | SSW |
| Tuesday | 9 | 33 | 44 | 30.19 | 30.19 | NW | W | Friday | 9 | 37 | 46 | 30.10 | 30.05 | SW | SW | Monday | 9 | 48 | 51 | 30.18 | 30.29 | SW | NW |
| Wednesday | 10 | 30 | 40 | 30.22 | 30.16 | WbN | WSW | Saturday | 10 | 41 | 48 | 30.00 | 29.97 | SW | WNW | Tuesday | 10 | 35 | 50 | 30.25 | 30.06 | SE | WbS |
| Thursday | 11 | 38 | 47 | 29.95 | 29.88 | WbS | SW | Sunday | 11 | 35 | 39 | 30.20 | 30.28 | N | NNE | Wednesday | 11 | 35 | 43 | 30.18 | 30.16 | W | NW |
| Friday | 12 | 38 | 41 | 29.65 | 29.61 | W | W | Monday | 12 | 26 | 35 | 30.33 | 30.33 | W | SW | Thursday | 12 | 31 | 48 | 29.80 | 29.47 | SW | SW |
| Saturday | 13 | 33 | 38 | 29.80 | 29.98 | W | NW | Tuesday | 13 | 26 | 33 | 30.37 | 30.34 | W | W | Friday | 13 | 34 | 42 | 29.09 | 28.90 | SW | SW |
| Sunday | 14 | 26 | 38 | 30.10 | 30.06 | W | WSW | Wednesday | 14 | 26 | 33 | 30.30 | 30.28 | W | NNW | Saturday | 14 | 33 | 39 | 28.84 | 29.00 | NE | NE |
| Monday | 15 | 35 | 41 | 30.08 | 30.12 | W | W | Thursday | 15 | 30 | 36 | 30.30 | 30.30 | WNW | NW | Sunday | 15 | 32 | 41 | 29.37 | 29.40 | N | NNE |
| Tuesday | 16 | 30.5 | 45 | 30.00 | 29.79 | SW | S | Friday | 16 | 30 | 35 | 30.47 | 30.50 | NW | WbN | Monday | 16 | 24.5 | 36 | 29.48 | 29.39 | NE | ESE |
| Wednesday | 17 | 37 | 48 | 29.50 | 28.80 | SW | S | Saturday | 17 | 25 | 35 | 30.46 | 30.38 | W | W | Tuesday | 17 | 27 | 38 | 29.30 | 29.27 | SE | ESE |
| Thursday | 18 | 33 | 39 | 29.29 | 29.34 | W | W | Sunday | 18 | 33 | 40 | 30.32 | 30.28 | W | W | Wednesday | 18 | 31 | 42 | 29.20 | 29.18 | ESE | ESE |
| Friday | 19 | 29 | 34 | 29.52 | 29.60 | WNW | WbN | Monday | 19 | 34 | 44 | 30.12 | 30.08 | NW | W | Thursday | 19 | 34 | 45 | 29.28 | 29.28 | ESE | ESE |
| Saturday | 20 | 28 | 44 | 29.14 | 29.00 | SE | WbS | Tuesday | 20 | 39 | 43 | 30.03 | 29.92 | SW | WbN | Friday | 20 | 37 | 49 | 29.38 | 29.48 | E | E |
| Sunday | 21 | 32 | 41 | 29.13 | 29.20 | SW | SW | Wednesday | 21 | 38 | 44 | 29.53 | 29.41 | SW | WSW | Saturday | 21 | 25 | 42 | 29.59 | 29.62 | W | W |
| Monday | 22 | 38 | 41 | 29.50 | 29.68 | NW | NW | Thursday | 22 | 35 | 42 | 29.19 | 28.90 | W | S | Sunday | 22 | 32 | 45 | 29.74 | 29.74 | E | E |
| Tuesday | 23 | 33 | 44 | 29.63 | 29.65 | SSW | WNW | Friday | 23 | 38 | 44 | 28.70 | 28.80 | WSW | NW | Monday | 23 | 37 | 41 | 29.80 | 29.83 | E | |
| Wednesday | 24 | 41 | 48 | 29.80 | 29.85 | SW | SW | Saturday | 24 | 34 | 47 | 29.53 | 29.69 | W | W | Tuesday | 24 | 37 | 39 | 29.83 | 29.78 | E | |
| Thursday | 25 | 46 | 49 | 29.83 | 29.73 | SW | SW | Sunday | 25 | 40 | 50 | 29.72 | 29.84 | WbS | SW | Wednesday | 25 | 33 | 36 | 29.70 | 29.70 | NE | NE |
| Friday | 26 | 40 | 46 | 29.70 | 29.48 | SW | SW | Monday | 26 | 47 | 52 | 29.86 | 29.90 | SW | SW | Thursday | 26 | 30 | 40 | 29.68 | 29.64 | NbE | NE |
| Saturday | 27 | 42 | 44 | 29.10 | 29.11 | SW | WSW | Tuesday | 27 | 42 | 50 | 29.67 | 29.61 | SW | W | Friday | 27 | 33 | 34 | 29.55 | 29.52 | NE | NE |
| Sunday | 28 | 36 | 49 | 29.35 | 29.10 | SW | S | Wednesday | 28 | 35 | 42 | 29.40 | 29.38 | WbS | WbN | Saturday | 28 | 34 | 39 | 29.62 | 29.67 | NE | NE |
| Monday | 29 | 39 | 46 | 29.00 | 28.95 | SW | WSW | Thursday | 29 | 34 | 41 | 29.70 | 29.80 | WbN | NW | Sunday | 29 | 33 | 37 | 29.77 | 29.78 | NE | NE |
| Tuesday | 30 | 39 | 45 | 28.90 | 29.06 | WbS | W | Friday | 30 | 25.5 | 37 | 29.93 | 29.91 | NW | S | | | | | | | | |
| Wednesday | 31 | 38 | 45 | 29.35 | 29.54 | W | W | Saturday | 31 | 33 | 43 | 29.80 | 29.80 | S | S | | | | | | | | |